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THE ROLE OF PROJECT HISTORIES IN THE ST

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January 1964

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THE ROLE OF PROJECT HISTORIES IN THE STUDY OF R&D

I. INTRODUCTION

The student of research and development today finds himself, though no longer alone, still very much in an uncharted territory. The importance of knowledge about the process that generates knowledge no longer needs to be argued. The national research and development effort, as a necessary condition of technical change and therefore of economic growth, is taking its place among the government's major economic policy opportunities. Efficient management of government financed R&D is a growing challenge.

But sound, accepted knowledge about the R&D process is still extremely scarce. This is true even if we ignore the basic research end of the R&D spectrum, as we shall do here, and concentrate only on development proper. By "development," or "the task of a developer," we mean the achievement of a new and satisfactory item (aircraft, communications device, chemical process, missile, and so on) using the established principles that are the products of basic research. There are at least three ways in which knowledge of the development process can be acquired:

(1) The empirical study of aggregates. The R&D behavior of an entire industry, for example, is observed, as well as the consequences of this behavior. Changes in major variables -- total R&D budgets, numbers of patents, and so on -- are explained in terms of other variables (productivity, sales, industry structure, and the like) so as to fit the data well. Aggregate studies over all or many projects of a certain kind are also possible -- studies of aircraft projects or commercial aircraft projects, for example, or studies of chemical projects or petrochemical projects. Such studies would first attempt to accumulate data -- for example, on the time paths of expenditures in the projects, or on the rates at which certain kinds of learning occur. With luck there would be enough data to permit some hypotheses to be tested in a statistically meaningful way.

(2) The development of a normative theory in which the properties of the "good" conduct of development are deduced with some rigor from precisely stated assumptions about the nature of development and the goals of the developer.

(3) The intensive historical study of completed development projects.

In the course of studying research and development, RAND has engaged to some extent in all three approaches.* We shall be concerned in this study, however, only with the third approach. The compilation of a number of detailed historical case studies began at RAND some years ago but because of security considerations only a few studies have been issued to the general public.** This Paper is based on a group of histories of postwar military development projects written at RAND between 1956 and 1958. Our purpose here is to illustrate the project history and the sorts of conjectures about development on which a group of related histories can shed light.

The project histories with which we shall deal are histories of airborne radars, aircraft engines, fighter aircraft, bombers, and an air-to-air missile. When sufficient in number and sufficiently closely related, a group of histories can provide strong support for general conjectures about the nature of development, and specific conjectures about the consequences of (1) alternative strategies for the conduct of development in a given area of technology, and (2) alternative modes of organizing the team of people who make up a development project.

Project histories, on the other hand, have several serious limitations. The main one is that a strong subjective element often enters the interpretation of a history and the decision as to whether or not

* In the first category see, for example, A. W. Marshall and W. H. Meckling, Predictability of the Costs, Time, and Success of Development, The RAND Corporation, P-1821, December 11, 1959. In the second category fall a RAND Memorandum by R. R. Nelson, The Economics of Parallel R&D Efforts: A Sequential-Decision Analysis, RM-2482, November 12, 1959, and some work by the present writer, not yet available for distribution.

** For example, R. R. Nelson, The Link Between Science and Invention: The Case of the Transistor, The RAND Corporation, P-1854-RC, December 15, 1959, and T. A. Marschak, Strategy and Organization in a System Development Project, The RAND Corporation, P-1901-RC, February 3, 1960.

it supports a given conjecture. This is, of course, the classic difficulty of all historical analysis.

We shall illustrate here both the possibilities and the limitations of the project-history method. To do so we shall focus on two main topics and shall investigate the extent to which our collection of case histories can support a hypothesis about each of them. First we shall try to see whether the histories support the hypothesis that major uncertainty, at least in the earlier phases of development, is a natural and inevitable property of a program that seeks a real technical advance.

Second, we shall try to see whether the histories support a hypothesis about the consequences of making heavy or light commitments in the early phases of development. Stated very broadly, the hypothesis is as follows: When the predictions available at the start of a development program are used as the basis of heavy commitments to a highly specified design, then in the fortunate cases when the predictions turn out to be correct, time and money may be saved if such commitments do not have to be postponed until later in development. If the predictions are seriously wrong, however, costly revisions will be required. The initial uncertainties of development are such that the gains due to heavy initial commitments in the fortunate cases are outweighed by the costly revisions of the unfortunate cases.

Both topics and both hypotheses take different concrete forms for the several different areas of technology with which we shall deal. We will begin by presenting the histories. The hypotheses will be stated in detail and examined in the concluding section.

The studies that follow all deal with military projects because all of them were prepared as part of a broad effort to understand military research and development.* For several reasons the studies

*In addition, the two nonmilitary studies cited above have already appeared (P-1854-RC, P-1901-RC).

differ from each other in scope and style. The original studies, on which the sections that follow are closely based, were prepared by several different authors (as indicated at the beginning of each section). Furthermore, the availability of historical material varied considerably; some projects were voluminously documented while others required patient interviewing and searching through files to construct even a fragmentary history. Again, the original studies differed with respect to the amount of material that had to be deleted here because of (a) military security or (b) the privileged status of the material (revealed in confidence by private firms).

The following histories contain a fair amount of historical and technical detail. The aim is to preserve, as much as possible, the true flavor of the histories, the concrete form taken by development strategy, and the uncertainties of development.

Finally, a strong word of caution is in order about the interpretation of the histories. The criticism of past development procedures, or the past performance of any development agency, is not our purpose. Nor is our purpose to advocate one kind of R&D management policy as opposed to another. Any such interpretation of the histories entirely misses the point of this Paper: to illustrate an important method for acquiring knowledge about the research and development process.

II. AIRBORNE RADARS

(Based on a Study by B. H. Klein and E. Sharkey)

In the development of a military airborne radar, the final item is an operational radar, satisfactorily performing in one or more specific aircraft in a specified military environment (combat, reconnaissance, and so on). We shall consider several programs that differed in important respects.

"SIDE-LOOKING" RADAR

Development History

Early in 1954 some reconnaissance radar photographs taken by the Royal Air Force were seen at the headquarters of the Strategic Air Command. The radar used was a "side-looking" radar, photographing a strip several miles wide on either side of the plane. The reflected radar signals were received through fixed linear antennas mounted on either side of the plane. Previous radars had photographed a circular area, using a rotating antenna. The side-looking radar operated at a frequency in the K_a-band.* Its photography represented a startling improvement over previous radar photography; in information content it approached the quality of poor-resolution optical photography. At that time there happened to be an acute need for an all-weather, high-resolution reconnaissance capability to be used for the detection of air bases. The principal existing reconnaissance radar -- the APS-23 -- rarely met the need. Moreover, no U.S. Air Force radar then under development was capable of results at all comparable to the RAF photography.

In February 1954, personnel from Wright Air Development Center and from SAC went to Britain to examine the British photography and equipment in more detail. On March 25, SAC officially requested the Air Research and Development Command to initiate a 90-day program to develop a side-looking radar capable of detecting runways of a certain

* A frequency band in the neighborhood of 35,000 megacycles per second.

width from an altitude of 40,000 feet, considerably higher than the altitude from which the RAF pictures had been taken. On April 11, the Commander of the Air Research and Development Command ordered this program started on a high priority.

Early in May, Westinghouse Electric Company was given an informal go-ahead with the understanding that one K-band radar set would be designed, built and installed in a SAC reconnaissance bomber -- an RB-47 -- within 60 days. (The 60-day period was set to comply with SAC's earlier 90-day request.)

Although the British model might have been copied to meet this short deadline, for several important reasons it was not. First, SAC wanted a set that would operate at much higher altitudes than the British design; second, the system had to be designed to fit in an RB-47. There was no certainty that K_a-band radar could "see" all the way to the ground from 40,000 feet, and the question of K_a-band performance degradation by weather was still unanswered. It was necessary to get answers to these and other basic questions from the initial model, and quickly. Accordingly, the requirements put on the contractor were simple: to make a set that would detect (but not necessarily provide detailed photographs) runways of a specified width from 40,000 feet out to a range of 10 miles on at least one side of the aircraft. The flight test would show whether these requirements were met or whether it was possible to meet them. No other more detailed requirements were insisted upon, and no sophisticated features were asked for. The Air Force expected the contractor to come up with as good and reliable a set as was possible in the short time available, but there was no insistence on equipment details. The first model looked to only one side of the aircraft; to make it look to both sides would have increased the time required to achieve a working set.

The Air Force's arrangement with Westinghouse, concluded on May 15, took the form of an amendment to an existing Air Force-Westinghouse contract, and had several unusual provisions. In the

first place, specific compliance with JAN Specs (Joint Army-Navy Specifications) was not mentioned in the contract. Such specifications, often assigned at this stage of development, would have constrained the dimensions, performance, and reliability in a detailed manner. The informal understanding was that Westinghouse would choose components with an eye to making the equipment highly reliable and maintainable; and that the particular choices would require approval only of the Air Force people directly in charge of the project. Second, the contractor was given the task of maintaining the equipment for one year.* Third, the contractor was given responsibility for installing the radar in a SAC-furnished RB-47, and for working directly with Boeing (the plane's manufacturer), to make structural changes in the airplane so that it could accommodate the antenna.

The radar was designated the AN/AFQ-36. The total amount provided in the initial contract for the development, installation and maintenance of the first model -- the XA-1 -- was \$370,000.

Although the XA-1 did benefit considerably from the British side-looking radar work (and also from Westinghouse's previous experience in K_a-band radar work), major development problems remained. To get a 10-foot linear-array antenna to perform satisfactorily, for example, required an antenna design and construction effort that continued far beyond the time the XA-1 was first tested. Because side-looking radar involves an entirely different type of presentation than does a scanning (rotating) radar, new problems were faced in getting a satisfactory recorder and camera, as well as the usual problems encountered in making the first model of any kind of new radar.

Despite these difficulties, however, 59 days after Westinghouse was told to go ahead, the XA-1 was test-flown in the SAC RB-47, and

* Giving the contractor the maintenance responsibility undoubtedly made it easier to sell the idea of waiving technical specifications. The presumption was that if the contractor had to maintain the equipment he would not be likely to use components that would complicate the task of keeping it in good working order.

succeeded on its first flight in picking up runway patterns from the required altitude. The antenna pattern was not satisfactory, but the initial flights of this first model showed that at 40,000 feet operation was feasible, that runways were easily detected, and that weather was not much of a problem in the K_a-band.

Shortly after the XA-1 was flown, and after bids had again been solicited from three contractors, Westinghouse was awarded a contract for building ten improved models (the XA-2). The schedule called for the first delivery to be made on December 1, and for one additional set to be delivered every three weeks in the period following. Westinghouse was to deliver the sets directly to Lockbourne Air Force Base and install them in SAC RB-47s.

Westinghouse then proceeded to redesign the radar. A more complete system, looking to both sides of the aircraft, was designed and developed, and a number of improvements were made (for example, improved signal-to-noise ratio, smoothing of the antenna pattern). Other possible changes, for example, to insure better mapping accuracy, to get a larger film size, or to get in-flight processing of the side-looking radar photography, were not incorporated in this second design; the emphasis was still on quickly giving SAC a small, all-weather, high-resolution reconnaissance capability, now that it had been shown to be feasible.

It turned out to be very fortunate that no attempt was made to incorporate these peripheral improvements and, consequently, to delay testing, for when the radar was tested early in December, it was found -- as it has been found in the course of almost every airborne radar development -- that there were some fairly basic difficulties (for example, reliability, antenna pattern, and so on) that still had to be overcome. A series of modifications (resulting in the XA-2 Model II) was then undertaken to correct the difficulties and to make the radar operationally more useful. (SAC asked for a larger film size and a compass heading repeater.) In the main, however, improvements were aimed at overcoming the technical difficulties encountered

in the Model I version of the XA-2. The already delivered models were modified by being run back through the factory.

The first Model II set, which was flown early in May 1955, performed much more satisfactorily. At SAC's request, attention was then turned to increasing the operational suitability of the radar. Changes were made to improve the radar's mapping accuracy and to add other features that the tests showed to be necessary. The Model III was delivered for testing in September of 1955.

During the latter part of 1955 the airborne reliability of the equipment was improved to about 80 per cent. By the standards of postwar airborne radar development this is a rather remarkable achievement, especially considering that modifications were still underway during the period. (The malfunction rate of approximately 20 per cent includes the effects of shakedown flights on factory modified equipment.)

Organizational Aspects

The side-looking radar project had important organizational properties. The test and development programs were run as one, with a very wide measure of decisionmaking authority and responsibility vested in only a few on-the-spot Air Force people: two from Wright Air Development Center's Aerial Reconnaissance Laboratory, and two officers at Lockbourne Air Force Base. Higher echelons did not have to be consulted on changing a detailed "General Operational Requirement" or a previously prepared "Development Plan," because there were none. The contractor had responsible engineer/ personnel at Lockbourne AFB, where the test program was being conducted, and which was only a short distance from WADC. This facilitated quick decisions. By virtue of these arrangements the problem of communication between the operational command, the R&D command, and the contractor was minimized.

The integration of the test program into the development work not only permitted modifications to be made on the basis of realistic

test results, but it also gave SAC a considerable amount of operational experience in the use of a new technique. This seems to have been an important reason why the developed radar was attained with such relative speed.

The contractor displayed an enthusiasm and effectiveness that Air Force personnel reported to be exceptional. This cannot be attributed to any definite assurance of a large procurement contract for this radar, for the original commitment was for only ten XA-2s. Moreover, Westinghouse had no reason to regard itself as a unique source for side-looking radars. (Concurrently, Bell Laboratories was testing a side-looking radar of another frequency in a SAC plane, with very good results.) On the other hand, Westinghouse certainly did have reason to believe that there might be a substantial market for such a radar if it could be successfully developed, and, indeed, subsequently received a contract from the Navy.

It seems reasonable to assume that the organization of the project stimulated the contractor's eagerness to do a good job. Since it was able to get quick and relatively well-informed decisions, the company was able to keep its engineering staff fully and productively occupied from the time development on the XA-1 was initiated to the completion of the ten test models (the XA-2 Model III) of the Q-56, and thereby to keep the cost of the program low. There were no lulls in development activity while equipment was waiting six months or more to be tested. There was no long period of uncertainty between the completion of Phase I development (the XA-1) and the award of a Phase II contract (the XA-2). The blanket exemption from "JAN Specifications" made it unnecessary for the contractor to apply for necessary exemptions piecemeal, and to wait the customary interval (in some programs six months or so) for approval. (Approximately 60-70 per cent of the Q-56 parts were not on the approved JAN list; 20-30 per cent actually had never been built before.) Long periods were not spent ironing out differences among the contractor, the R&D people, the agencies responsible for testing the radars, and the operational command.

WORLD WAR II RADARS

During World War II at least five different USAF airborne microwave radars were developed to a quite adequate degree of operational usefulness and reliability; and were used extensively in combat.* These radars were developed similarly in important respects to the strategy used in the Q-56 program. The need for each of the radars was urgent. Their development was not based on any elaborate requirement. There was no long-term, highly detailed consideration of all the factors of the operational environment in which they were going to be used; a "best" set of specifications for meeting this environment was not imposed. Rather, several radars (often of different design) were quickly readied for flight testing. After testing, one or more was chosen to be brought quickly to combat use† status. A small-scale combat operations test was usually the final phase. Production of a final model then began. The argument for quickly getting models into a highly realistic test environment seemed to be that there was simply not time to do otherwise. It is true, of course, that some of the accelerated test programs turned out poorly; but a radar that was not good enough was discovered quickly.***

The development of the SCR-717-BB radar along with the APQ-5BB computer for low-altitude bombing is an example of such a program. Development of the radar and computer as experimental models was begun in 1942. In August 1943, following a quick R&D flight test, a realistic one-squadron combat flight test program was initiated (the test area

* Some other radars were developed through the initial testing phase, and then cancelled.

** In so characterizing wartime radar development, we certainly do not want to imply that all the wartime radar programs were conducted in exactly the same manner. Not all the models that were tried in combat tests were "development" models, some were "research" models. The line between research and development was at times hard to define. But the tendency to push even very crude experimental models into operational aircraft was one important difference between the wartime and later conduct of radar development.

happened to be in the Solomon Islands), during which many modifications, both in the radar and the computer, were made. Within five weeks, the reliability of the radar and the computer on combat missions was improved to over 90 per cent.* It is worth noting that the equipment was used in fair quantity for the rest of the war with no further changes (except for the switch from "S-band" to "X-band" in the radar when this became possible).

The SCR-517 series of radars (the first USAF microwave radar) was used primarily at low altitude in the antisubmarine campaign. The APS-15, the APQ-13, and the APQ-7 were high-altitude bombing radars; they were basic radar sets with a very simple bombing computer included. Figure 1 shows the development times for all of these wartime microwave radars.

SOME POSTWAR RADARS

For postwar radars completed by 1957 other than the Q-56, the time elapsing between the start to development and the start of operational use ran from about four to seven years.** Most of these radars proved to be far from satisfactory. Where the radar in question was already in production, correction of the faults was difficult and expensive.

The APS-23 is one example of a radar that took a long time to develop and was found to be unsatisfactory. In terms of inherent technical difficulty, this radar should not have been essentially

* Radar reliability was then defined more stringently than became the case after the war: a flight was "successful" only if the radar functioned satisfactorily during the entire flight.

** The bombing radars developed include the APS-23 (part of the K- and Q-24 bombing systems), the HSBK (part of the MA-2 bombing system), the APS-64 (a tunable modification of the APS-23), and the K-5 radar (part of a bombing system developed for the Tactical Air Command). Although the associated computers had the same problems as the radars, only the radars are discussed here because the radars are all roughly comparable as a development problem. The all-purpose search radars were the APS-42 and the APN-59. (The APS-42 was actually a Navy design which has been widely used by the Air Force.)

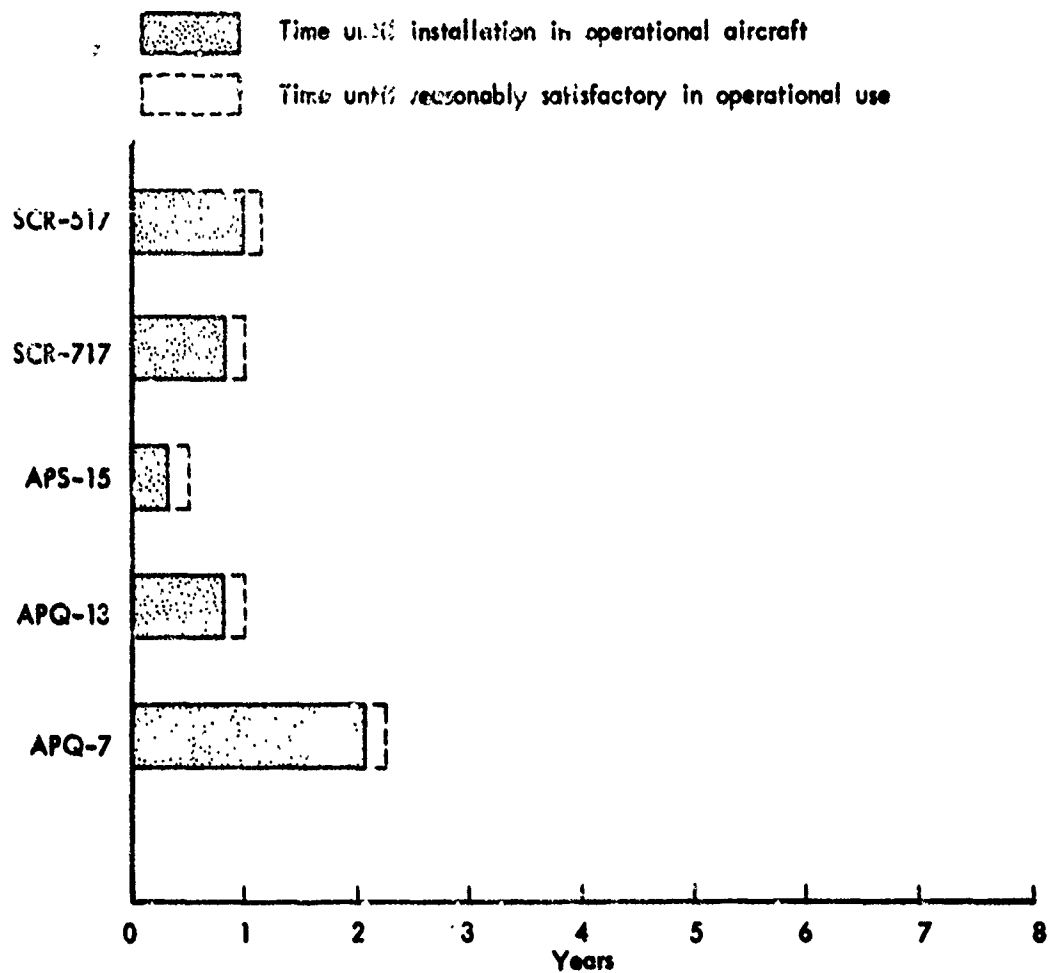


Fig. 1 — World War II radar development

more difficult to develop than a number of other radars, including the Q-56. Design of this radar began in 1945. It was first put in SAC planes in 1949, four years later, but after a relatively insignificant amount of testing (compared with the Q-56). It was combined with a computer to form the Q-24 bombing system but the immediate impact of the introduction of the Q-24 was a significant loss in SAC's combat capability -- the system simply did not work often enough. Two years after being installed in SAC airplanes, and a full six years after the initiation of the program, many faults of the radar (and the associated computer) still had not been overcome. A major program (Project Reliable) was undertaken to bring the performance of the Q-24 computer-radar system up to acceptable standards. Within half a year, a limited number of sets assigned to a special test and modification program were brought up to a reliability of over 90 per cent; but it was some time later before all of SAC's sets were appropriately modified. The cost of the modification program per radar system exceeded the original purchase price of the equipment.

There was similarly slow progress in the case of the K-bombing system, which also used the APS-23 radar but with a different computer. From the first installation of the K-bombing system in SAC aircraft to the achievement of a 10-15 per cent failure rate took about five years. During this period the APS-23 radar was responsible for roughly half the failures. Many factors contributed to these poor performance difficulties, but it seems safe to say an important factor was the insignificant amount of testing the systems received prior to installation.

As a result of the unsatisfactory experience with these two radar bombing systems, interest became focused on equipment reliability in the development of the next radar bombing system. (This was the MA-2 system, previously called the HSBD, and later the ASB-4; the radar portion is called the HSBR.)

The HSBR took more than six years to develop. Flight experience began late in the program. It was then necessary to modify the MA-2

radar and computer extensively in order to satisfy SAC with respect to operability and reliability, even though reliability had been heavily emphasized from the start. SAC's dissatisfaction with the system developed soon after SAC personnel got their first real flight experience with the system.

The other postwar radars have not had as serious problems when they were introduced into operational use, but for the most part these radars represented relatively small departures from existing equipment. The relative speed of the Q-56 program is shown in Fig. 2.

The development histories of a number of postwar radars contrast sharply with those of the Q-56 and some of the World War II radars. For these postwar programs the aim was to avoid successive modifications and to proceed directly, by careful planning, to the final production models. Detailed requirements were issued before the start of development. The requirements were determined after considering in detail the capabilities of the radar observer; the logistical and training difficulties, and what was then known about the performance of alternative possible designs. Following the issuing of detailed requirements, a detailed design proposal and a detailed development schedule were approved by the appropriate agencies.

Since the approved development plan and the approved requirements were the result of such an intensive effort, it was hoped that major design changes would not occur in the course of development. When they did appear necessary, the organization of the program often made it difficult to get them approved. Responsibility for the project was divided into a number of functions (for example, formulating the initial requirements, programming the development effort, supervising the program, testing the development equipment) which were performed by a number of different specialist agencies. Many design changes required the concurrence of all of them, and this, of course, took time.*

* The best evidence for this characterization of postwar development is Manual 80-4, issued by the U.S. Air Research and Development

Much of the testing effort in the postwar programs consisted of lengthy series of "R&D tests." Members of the operating command (the final customer) played only a minor role in these tests, as compared with the Q-56 and the wartime programs. The R&D tests were generally not as realistic as those of the wartime programs.

INTERPRETATION

What tentative explanations can be proposed for the relatively short development time of the Q-56 and the World War II radars? Were they perhaps (1) "easy" development jobs, or (2) projects in which short development time was bought by the expenditure of an unusual amount of money?

(1) Ease of development. Whether or not a given program was "easy," or included a small technological jump by comparison with other programs, is perhaps the most delicate problem with which the student of project histories has to deal. Generally one has no choice but to rely on the personal opinions of technical experts.

We suggest that there would be substantial agreement with the following assertions:

(a) The many advances incorporated into operational airborne radars during the few years of the war far overshadow the relatively

Command, ARDC Program Management Procedures, August 1957, to define approved development procedures in Air Force-sponsored projects. The Manual describes the many steps (involving a number of agencies) that must precede issuance of a development contract, as well as the procedures required in obtaining approval for subsequent changes. The Manual interprets these procedures as follows:

"This concept of performing work in accordance with a plan sets up an implicit requirement that no individual or agency will make a change affecting time schedules, technical and performance requirements configuration, physical characteristics, or cost, without consulting with other agencies concerned and obtaining full agreement and approval. The approval of the original R and D plan validates the technical approach and the resources required but does not authorize initiation of work. Consequently, an agency must not change its commitment and must not undertake any action deviating from the approved plan until all participating, coordinating and approving agencies have been notified, the over-all impact of the proposed change evaluated and a decision in the form of a changed directive issued."

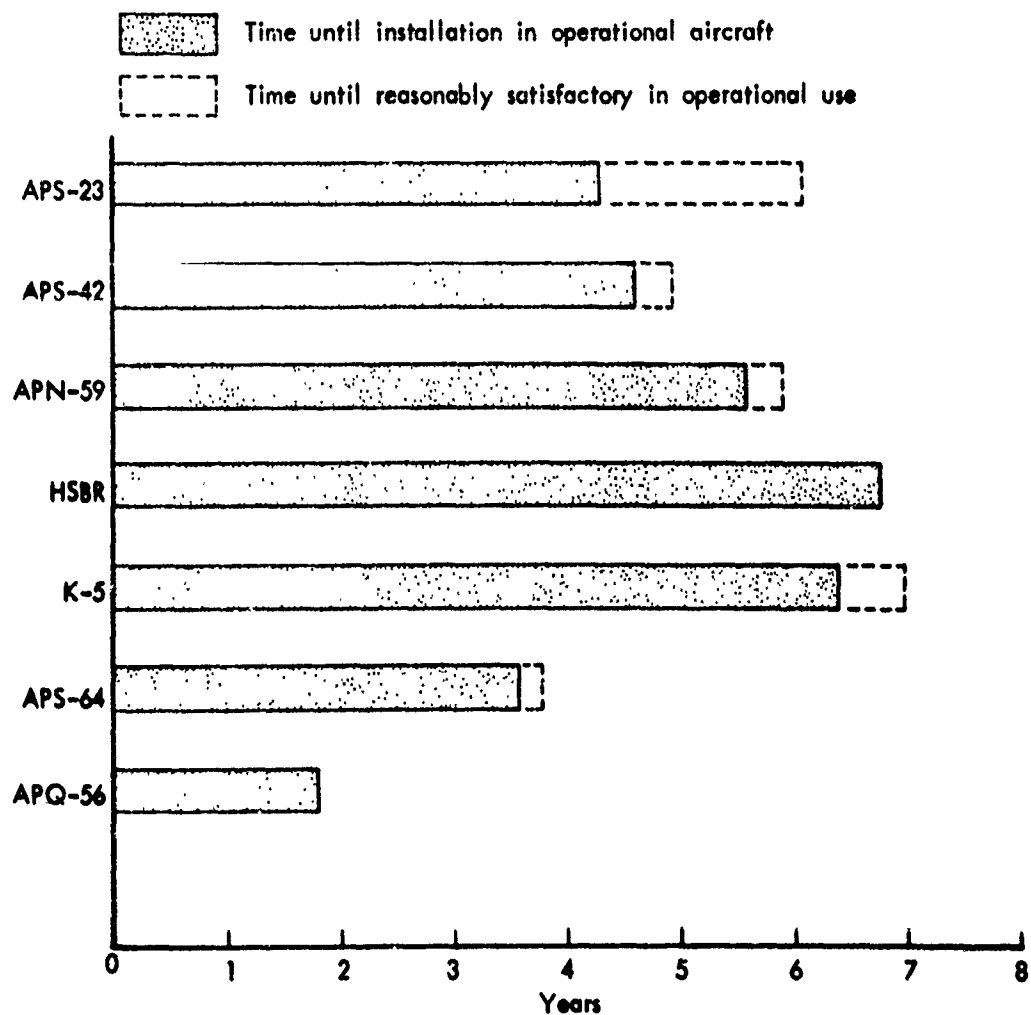


Fig. 2 — Postwar radar development times

few real advances that have been made in the decade following the war. The primary difference between the wartime and the postwar airborne radars is that the latter are better integrated into the associated bombing computers; however, the extra complexity has appeared primarily in the computers, not in the radars.

(b) Although the Q-56 did benefit considerably from the experimental flight test work of the British, it can hardly be characterized as a "relatively easy, off-the-shelf, components-assembly" job." A number of the main components were unusually large departures from their counterparts in preceding radars. The novelty reflected the fact that the Q-56 was to operate in a frequency range in which there had been little relevant experience; that it was not to be a scanning radar with rotating antenna like previous radars; and that unlike other radars, which could supply information directly to an observer, it would be useless without very high-speed photography. In particular an antenna and lens system different from the British one had to be designed, and the construction of the K-band linear-array antennas to the needed tolerances proved to be an especially difficult job.

(c) Though the other airborne radars developed since World War II have all had special problems, their development has not involved many radically new techniques. The basic design of the APS-23, for example, was completed during the war.

(2) Development costs. Approximate development costs for a number of wartime and postwar airborne radars are given in Table 1. The figures in the first column relate to the actual development costs; the figures in the second column have been adjusted for increases in engineering and materials costs in order to indicate their approximate price levels.* It will be noted that in presenting the cost of the wartime programs, we have lumped together as a single program those radars that were developed from the same general design.

*These adjustments were made on the basis of contractor-furnished information on wage and material costs.

Table I
RADAR DEVELOPMENT COSTS^a

Time Period and Radar Problem	Actual Cost (million \$)	Approximate Cost in 1957 Dollars (million \$)
<u>Wartime</u>		
SCR-517-19-20	3.5	7.2
SCR-717 A, B, C	3.2	6.6
APQ-13	3.0	6.3
APQ-7	2.5	5.3
<u>Postwar</u>		
APS-23	8.0 ^b	11.1 ^b
WSBR	15.6	17.2
APQ-56	5.1 ^c	5.3 ^c

Notes:

^aIncludes cost of fabricating initial flight test models, service tests, and subsequent engineering services.

^bDoes not include Project Reliable costs.

^cIncludes the cost of contractor maintenance and the cost of SAC's ten operational radars.

The table shows that the wartime "emergency" programs were not expensive compared with the postwar radar development program. On the contrary, in terms of 1957 engineering and materials costs, the cost of developing the wartime radars averaged not much more than half the cost of the APS-23 program, and not much more than one-third the cost of the HSBP program.*

Ease of development and money spent do not, then, explain the speed of the Q-56 and the wartime programs. A strong contender as an explanation is the set of differences between the general development strategy of the World War II and Q-56 programs on the one hand and the development strategy of the indicated postwar radars on the other. Organizational differences between the two groups of programs supplement this explanation.

In the wartime radars and the Q-56, detailed operational and technical requirements were absent. The initial emphasis was on getting out preliminary flight test models quickly. The test program for these models, moreover, consisted of a rapid, concentrated series of realistic tests; equipment was flown and tested by the operating Air Force command in the same environment in which the final item was to perform.

These properties of the program seem to have had the following consequence: The absence of detailed requirements made it possible to get flight test models quickly. It was not necessary to spend time ensuring that a flight test model was consistent with requirements

* In making the cost comparisons we did not include in the cost of the wartime radars the amounts spent for research and initial development work on them by the M.I.T. Radiation Laboratory. The above figures cover costs on development work done on getting radars designed for production for use in combat aircraft; they do not include research program costs. If the cost of Project Reliable were adjudged to be a development cost, the total APS-23 development costs would be substantially larger than shown above. It might be noted, however, that even including amounts spent at the Radiation Laboratory on the wartime radars, which would increase the wartime amounts shown by about one-third, the wartime programs still stand out as costing less than the postwar programs (in which no research costs are included).

imposed on the final model. The speed with which the test data were in fact obtained, and their realism, made it possible to determine the faults of the tested models very quickly -- faults that are unpredictable, inevitably occur, and can only be spotted through realistic testing. (The cost of obtaining this information was a small fraction of the total development cost.) Correction of these faults was quick and inexpensive at this stage of development, as compared with modification of models already in or near the production stage. Moreover, the broad definition of a "satisfactory" version of the radar permitted a wide choice of corrections to be made.

Organizational aspects of the Q-56 and wartime programs seem to have reinforced these effects of the strategic properties. Responsibility and authority were held by a small group of on-the-spot people. Changes in the programs were approved very quickly. Those in charge of operational matters were quickly able to obtain information and advice about the equipment from the developing engineers.

The strategy and organization of the postwar programs, on the other hand, was, as we have shown, quite different. Commitments were made to detailed designs and detailed schedules at a stage when knowledge about designs was unreliable. Realistic early testing was not stressed and there were organizational obstacles to the quick approval of major advances.

III. AIRCRAFT ENGINES

(based on a study by T. Marschak)

We shall report in this section on an investigation of turbojet and turboprop engine development histories. The engines in question were all completed in the period between the end of World War II and the end of 1957. Most of the histories that it proved possible to compile are brief and fragmentary. Only one is detailed.

Although the histories are deficient, we shall preface them with a largely illustrative attempt to characterize the general nature of engine development. We do not attempt such a characterization for any of the other areas of technology with which the project histories in this study deal, although such an attempt would be useful in all of them. The main hypotheses whose possible testing concerns us in this study have to do with the way the developer's knowledge changes as development proceeds and the performance of different development strategies, that is, different ways of responding to the changed knowledge. For each of the areas of technology we consider, it would be useful to have a model of the typical development project, in which the changes in knowledge that occur are described concretely. In such a model the main magnitudes characterizing the item emerging from the typical project are considered, and the learning that typically occurs with respect to each of them as development proceeds may suggest a natural division of the project history into stages.

There are a great many ways to divide development into stages. If, in each of the stages chosen, there are relatively great gains in the developer's knowledge about some of the magnitudes defining the final item, and relatively small gains in knowledge about others, it is easy to describe development strategies. Suppose it is in fact possible to define stages A, B, and C of the typical project such that there is relatively great reduction in the developer's uncertainty about magnitude 1 in stage A, about magnitude 2 in stage B, and about magnitude 3 in stage C. Then a strategy involving heavy commitments when uncertainties are still large could be described

as a strategy that bets heavily on predictions about magnitude 1 at the start of stage A, magnitude 2 before stage B, and magnitude 3 before stage C.

We shall make a rough attempt at a division of this kind for the case of engine development. The model is intended primarily to be an illustration of the sort of model it would be useful to have for any technology. Its realism in the case of many past engines is certainly open to question. The stages do not correspond to stages that have been generally defined for the purposes of official project administration, and, of course, they are not intended to. In actual practice the stages have often overlapped in time. They may also blend into each other so that it may be hard to say whether a given development belongs to the beginning of a certain stage or to the end of the preceding stage.* For a number of engine histories in the period with which we are concerned, however, the suggested sequence of stages seems a reasonable approximation, and for some of these the model will serve to make the characterization of the development strategy more concrete.

THE NATURE OF ENGINE DEVELOPMENT: AN ILLUSTRATIVE MODEL

General Observations

An engine is characterized by a number of magnitudes -- performance, weight, durability, unit production cost, spatial dimensions. The development of a new engine yields knowledge about the effort needed to attain certain values of these magnitudes. Spatial dimensions have an important peculiarity: an engine may be made larger or smaller within wide limits, that is, it may be scaled up or down, while preserving its basic internal design arrangements.

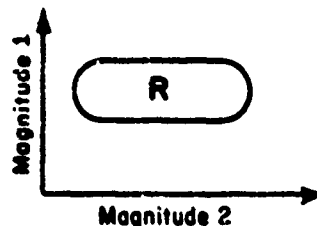
As development of an engine proceeds, there is less and less uncertainty about how difficult it will be to attain alternative

*The model's stages are particularly in need of modification for the case of engines in the Mach 3 range. Development of such engines got under way after the period with which we are concerned.

sets of values of the engine magnitudes. More precisely, the decline in uncertainty and the acquisition of knowledge during the course of development may be approximately described as follows:

(a) At a given point in the course of developing an engine of a given internal design, suppose that x dollars per week are available to the project for y additional weeks. Suppose also that the scale of the engine is approximately fixed. One then wants to make a prediction about the engine magnitudes that will characterize the engine at that scale when the x additional dollars have been spent and the y additional weeks have passed. Such a prediction may take the form: "the values of the engine magnitudes will lie in a certain region R " (a region in the space of these magnitudes). To such a prediction one may attach a degree of confidence -- say 60 per cent. This means, roughly, that one expects predictions made in such programs to be right 60 per cent of the time provided the region R is always chosen to have the same size. A higher degree of confidence can be attained by taking R to be a larger region; if R is taken to be the entire space of the magnitudes, then the degree of confidence is 100 per cent.

The statement that at a certain point in the course of development magnitude 1 is more accurately predictable than magnitude 2 then means that for any degree of confidence, and for any fixed values of x and y , the associated region R permits much less variation with respect to magnitude 1 than with respect to magnitude 2. If, for example, there are just two magnitudes (other than scale) then the region R lies in a two-dimensional space. The figure below indicates a situation in which magnitude 1 is more accurately predictable than magnitude 2.



R: region in which final engine magnitudes are predicted to lie, for fixed degree of confidence, fixed engine scale, and fixed remaining budget to be spent over fixed remaining time interval.

(b) Suppose, in the course of development, that one considers the effect of changing to a new engine size. Suppose also that for the old size engine, performance is very accurately predictable but the other engine magnitudes are much less accurately predictable. Then for the new scale, performance remains predictable with high accuracy but predictability of the other magnitudes may decline somewhat further. Performance predictions, in other words, can be scaled up or down without much influencing their accuracy; predictions about the other magnitudes lose in accuracy when they are scaled up or down.

(c) As development of an engine proceeds all predictions increase in accuracy.

(d) At any point in development, performance is generally more accurately predictable (for fixed engine scale) than the other magnitudes. Durability is often the least accurately predictable magnitude, until the very last stages of development.

An Idealized Division of Engine Development into Stages

We shall now describe the development of a new engine in terms of the following successive stages: (1) collection of "on-the-shelf" component test data without application to a specified complete engine (this stage may not occur); (2) the general design study stage; (3) the stage of performance-oriented development; (4) the stage of weight and durability oriented development; (5) the final prototype stage. Beyond the final prototype stage "operational development" may occur, that is, a long sequence of minor modifications that may be quite costly and are stimulated by operational experiences with the engine. But we shall consider development to be complete once operational engines exist. One or more of the stages (2), (3), and (4) may be performed by a developer for several alternative designs, each holding some promise of providing the new engine.

Within the second and third stages, a demonstration in the sense of a sharp increase in knowledge (predictability) of one or more engine magnitudes may occur. The demonstration may be achieved by running "demonstrator" engines (described below) or by testing critical

components, or both. The sharp increase in knowledge may take the negative form of determining that a running demonstrator engine or testable components can be built only at extremely great cost over a long period of time so that a decision may be made not to attempt the building but rather to drop the project.

Collection of "On-The-Shelf" Component Test Data

In several past engines it is possible to identify one or more components (for example, the compressor) that incorporate design innovations taken "off the shelf." Those innovations were contained in experimental components built prior to the inception of the engine and without reference to any specific using engine. Extensive test data obtained from the experimental hardware were then placed "on the shelf" to await the start of a complete engine in which components incorporating the same design innovations (but perhaps of a different scale) could be used. In providing performance predictions for such an engine, the on-the-shelf test data are very useful.

The General Design Study Stage

In this stage, the basic principles of a complete engine's construction are specified -- for example, whether it is to be axial or centrifugal flow, single rotor or dual rotor, fixed stator or variable stator, and so on. In addition, dimensions and shapes of the major components -- compressor, combustion chambers, turbine, and so on -- are tentatively specified. These specifications then also approximately imply the frontal area and over-all dimensions of the engine, though not the weight. The scale of an engine incorporating the basic principles, in other words, is chosen for further study. On the basis of these design specifications, using whatever on-the-shelf test data are available and relevant, and using thermodynamic cycle studies describing the family of theoretical thermodynamic cycles to which the engine's operation may be expected to correspond, performance curves for the engine are obtained.

These curves show specific fuel consumption versus thrust (or versus equivalent shaft horsepower (ESHP) in the case of a turboprop)* for alternative given air speeds, altitudes, and durations. Several points on this family of curves serve as standard numbers that are frequently used to characterize an engine, for example, maximum thrust (or ESHP) at sea level and zero speed for 5 minutes duration and for 30 minutes duration, maximum thrust (or ESHP) at 35,000 feet and 500 knots for 5 minutes and for 30 minutes duration. The coordinates of such points may be considered performance parameters roughly defining the performance curves. In computing the performance curves, it is necessary to consider other variables upon which the performance variables depend: for example, air flow, nozzle and inlet temperatures and pressures, and fuel flow. Curves relating these variables are also obtained to use in estimating how great are some of the problems of materials, of detailed component design, and of accessories.

The performance curves of the study yield fairly accurate performance predictions. Relevant on-the-shelf test data further improve these predictions. Alternatively, an engine can be built of the same basic design but of different scale; and for this engine, "scaling" of the original performance curves and the original on-the-shelf test data yields similarly accurate performance predictions.

Weight, durability, and unit production cost are much less accurately predictable at this stage than is performance because they depend on the solution of many individual problems of materials, of detailed component design and construction, and of accessory design and construction. The paper design studies of the variables underlying the engine's performance -- temperatures, pressures, airflow, and so on -- together with general knowledge about the state of the art for materials, components, and accessories, and data on the specific on-the-shelf components to be used, yield only very rough estimates of the time and money needed to solve most of these problems.**

*Pounds of fuel per hour per pound of thrust (or per equivalent shaft horsepower).

**In the design study stage, the developer may also pay attention to the accessibility of components for maintenance of the proposed engine. There may be preference for a design promising great accessibility.

The Stage of Performance-oriented Development

If the design innovations in the engine's major components have previously been extensively tested (though perhaps in a scale different from that of the final engine), so that a collection of on-the-shelf test data is available, then the stage of performance-oriented development may be very short and inexpensive. Its purpose is to achieve very accurate predictions of the performance of the complete engine, and the component test data when pieced together in the general design study stage may already have yielded such predictions for the scale chosen in the study.

If, on the other hand, no major component test data can be taken off the shelf and if the engine incorporates major design innovations, this stage may require considerable time (perhaps a year or more) and money. To achieve sharp increases in performance predictability beyond that of the general design study, one possible method is the building and running of (or the attempt to build and run) a performance-oriented demonstrator engine. This is an engine incorporating the design principles of the general design study but whose durability is only great enough to permit its running for a short time and whose weight may be quite high compared with the engine of the same scale that emerges at the end of development. Thus, none of the detailed problems of reducing weight and increasing durability has been attacked in building the demonstrator, and it may be built in any scale that is convenient for available test facilities. The performance data obtained by running it may be "scaled" so as to provide nearly as accurate predictions about performance for an engine of different scale as about performance for an engine of the demonstrator's scale. The performance predictions after a demonstrator is run can be expected to be quite accurate.

The same kind of sharp increase in performance predictability may be achieved by constructing and testing those components that have a major effect on performance and for which no test data exist. Generally, such components may be built to a convenient scale. This is the method that has prevailed in the engines studied. If the

uncertainty as to performance at the start of the stage hinges only on one or two major components of novel design, then it is probably quicker and cheaper to achieve the sharp increase in predictability by component tests than by building a complete demonstrator.

The Stage of Weight- and Durability-oriented Development

In this stage serious efforts are made to select materials of light weight and high durability for constructing the components of the chosen design.* In addition, there are numerous minute configuration details of the major components, and details dealing with the spacing, mounting, and connecting of the components that affect durability but do not significantly affect performance. Many of these details are worked out during this period.

In this stage, as in the previous one, a sharp increase in knowledge may be achieved by means of a demonstrator engine, component tests, or both. The engine or its components may be run in wind tunnels to simulate high air speeds and altitudes. In this stage, however, the increase occurs in knowledge of weight, durability, and unit production cost. (In solving weight and durability problems knowledge is gained of the cost of fabricating components out of the materials chosen and alternative materials.) The demonstrator and the test components differ markedly from those of the previous stage.

The demonstrator that may be used in this stage has the purpose of demonstrating that the design principles "work" (which means, generally, that performance is consistent with the performance data previously collected) for an engine of a certain scale, a certain weight, and a certain durability. Hence, it is possible to make predictions of much greater accuracy about the further reduction in weight and increases in durability that may be expected after specified further

* In turbojet engines in the Mach 3 range -- more recent engines than those considered here -- materials able to withstand high temperature are much more critical than in previously developed turbojets. Thus, serious consideration of the engine's materials problems may have to begin earlier, perhaps as early as the design study stage.

efforts. It may be possible to achieve the same result by testing components that have been constructed with materials problems in mind. If the uncertainty as to weight and durability at the beginning of this stage hinges on one or two major components only, then again it is probably cheaper and quicker to build and test those components rather than to build and complete a demonstrator engine.*

The scale of the demonstrator engine and the test components cannot be chosen as freely in this stage as in the previous one. Scaling up or down of weight, durability, and unit production cost predictions obtained from observing a demonstrator or from testing components of a given scale is much less accurate than scaling up or down of performance predictions. Hence the scale most likely to be "best" in the final engine is chosen with some care. Since the "best" scale depends on the changing knowledge mentioned several times before, and since the greater the accumulation of such knowledge supporting the scale decision the less likely it is that the decision will be regretted by the developer, he has a strong incentive to perform the weight- and durability-oriented development after the performance-oriented development (the order could conceivably be reversed). We shall not attempt to define the end of the stage of weight- and durability-oriented development except to say that it is the beginning of the final prototype stage.

The Final Prototype Stage

In this stage, a complete operational engine, meeting required standards of durability, is achieved. Accessories take final shape. Investigation of the major problems of tooling and fabrication that must be solved before quantity production may begin may also be started in this stage. In this stage, moreover, the design details (mounting arrangements, accessibility for maintenance, controls, and so on) that depend on the detailed configuration of the airframe to which the engine

*In the case of engines in the Mach 3 range, the predictions obtainable from a demonstrator engine are more limited since air speeds in this range cannot be simulated in existing wind tunnels.

is to be matched, are worked out and incorporated. To do so often requires some predictions as to the detailed configuration of the air-frame; and if these predictions turn out to be wrong, some of these "matching" tasks may have to be repeated.

Within the prototype stage, two milestones have generally been used: the passing of the 50-hour Preliminary Flight Rating Test and the 150-hour Qualification Test. Both of these tests are run according to precise military specifications. They are not run continuously but in a series of periods of specified lengths. Each period is further divided into segments in which specified conditions with respect to acceleration, thrust, control positions, fuel, lubrication, and so on must be met. The 150-hour test, in addition, includes numerous tests of separate components. After an engine has completed its running under test conditions, it is disassembled and the parts inspected to determine whether or not the test has been passed.*

Following the 50-hour test, there generally begins a series of tests in which the engine is run while flying in, though sometimes not powering, a testing plane. These tests are necessary in order to learn about the engine's durability at higher air speeds and altitudes than test facilities can simulate.

Passing the 150-hour test generally qualifies the engine for production. It is run under conditions of much greater variety and severity than the 50-hour test.

Between the two tests many highly detailed problems are solved. The prototype stage is generally much more expensive (though not longer) than the previous stages combined. However, the only recorded figures that bear this statement out with any precision relate to (1) the task of proceeding from the start of the design study stage (when there are no off-the-shelf test data for major components) to the passing of the 50-hour test, and (2) the task of proceeding from the passing of the 50-hour test to the passing of the 150-hour test.

* For Air Force sponsored engines, inspection has generally been performed by the Propulsion Laboratory of Wright Air Development Center.

In the period of this study the first task has taken something like one-third of the development costs and from 2 to 4 years; the second task has taken the remaining two-thirds of the costs and from 1 to 2 years.

Following the passing of the 150-hour test, the typical engine is by no means free from further alterations. Numerous troubles and deficiencies generally occur in operational use and, as we remarked above, "operational development" may occur through much of the engine's life. This may lead to separately identified later models of the original engine. The passing of the 150-hour test, moreover, may make it possible to predict with high confidence that the interval between overhauls of the average engine will be acceptably large; but the dispersion around this average interval is often very great.

In addition to "fixes" on the engine of the first model there are often subsequent models, incorporating design modifications, new materials, or additional accessories. The later models usually account for considerable improvement in performance, weight, and durability. Sometimes an engine may be developed of basically the same design as an existing engine but of different scale.

ISSUES TO BE EXAMINED IN THE ENGINE HISTORIES

In some engine development programs the developer's task was to achieve an engine meeting certain conditions; no using aircraft was specified. In other programs a using aircraft was specified; it is important to bear in mind that the complete development task in such a program is most accurately thought of as the achievement of a satisfactory airframe-engine combination. In either case one can investigate the commitments and choices made in the successive stages of development, and the knowledge that was available at the time. This we shall do, as far as possible, for the programs to be considered.

THE J-79

The J-79, which at the close of 1956 was nearing the end of its development (production of early models had begun), incorporates

important design innovations and represents a distinct advance. An unusual aspect of its history, compared with previous engines incorporating design principles of comparable novelty, is the accuracy of performance predictions (for the scale chosen) made after the first general design study. The accuracy seems principally due, as we shall see, to testing and placing its major critical component on the shelf prior to the development of the J-79 proper. The accuracy is indicated by Table 2.

The precise origins of the J-79 are both complex and obscure. Probably sometime in 1951 General Electric became aware that there was great Air Force interest in engines capable of speeds around Mach 2 whose specific fuel consumption was sufficiently low that high pressure ratio compressors were required. The Air Force interest became explicit when in February 1951, both Convair and Boeing were given contracts for design studies of a high altitude supersonic strategic bomber.

Two general engine (or compressor) design principles which seemed capable of producing the high compression ratios required were recognized at this time: the dual rotor with fixed geometry and the single rotor with variable stators. The latter principle had been unexplored in actual hardware (although long known in the literature). It offered the possibility of providing the same performance (for a given engine scale) at considerably less weight and for considerably less development effort than the dual rotor, since it was thought to be a less bulky and less delicate mechanism. Sufficient interest in the variable stator principle emerged at General Electric so that in June 1951, work was begun to modify a 14-stage, fixed-stator, single-rotor research compressor (with which unsuccessful attempts to achieve high pressure ratios had been made) into a variable-stator design. Six months later, enough testing of this compressor had been done to demonstrate by providing fairly accurate performance predictions that a variable-stator compressor would be capable of providing high pressure ratios and would not stall at low speeds. In late 1952, design study work was begun on a complete variable-stator engine -- a "demonstrator" engine from which the J-79 derived.

Table 2
ACCURACY OF PREDICTIONS AS COMPARED WITH
ACTUAL PERFORMANCE, J-79 ENGINE

	Predicted Performance of XJ-79, Autumn 1952 ^a	Official Performance (1956) of YJ-79-GE-1, First Production Model
Maximum thrust, sea level, static	14,346	14,500
Specific fuel consumption at maximum thrust (lbs/hr)	2.05	2.00
Military thrust, ^b sea level, static	9,442	9,600
SFC at military thrust	.86	.87

Notes:

^aIn addition to the comparisons shown, predictions made in a General Electric report dated June 1953, with respect to performance at high altitude, appear to have been substantially met by the first production model except that a slightly larger afterburner diameter than that predicted seems to have been required.

^bMilitary thrust, or military rated power, is the maximum power or thrust specified for an engine by the manufacturer or by the Air Force as allowable in flight under specified operating conditions for periods of 30 minutes' duration.

The dual-rotor approach was not yet rejected, however. A series of "paper engines" were shown to the Air Force after the testing of the variable-stator research compressor. In these proposals high pressure ratios were achieved by means of dual rotors. Within G. E. the dual-rotor approach still had strong advocates.

A month later, Convair was awarded a development contract for the B-58 with a General Electric engine of specified scale and performance. For several months afterward, there was still disagreement within G. E. as to whether the engine was to have dual rotor or variable stators. The variable-stator viewpoint finally gained ascendancy and the engine for the B-58 became the variable-stator J-79. Design studies for this engine were then completed. As a result of the data previously obtained from testing of the research compressor (on-the-shelf component test data), it was possible to predict with high confidence the performance that would correspond to the design scale chosen. (This scale was considerably smaller -- 165 pounds per second airflow instead of 240 pounds per second -- than the scale of the research compressor.) However, considerable uncertainty remained as to the levels of weight and durability (and unit production cost) that would be attained (for this scale) after given expenditures over given time periods. But it was believed that a given expenditure over a given time period would permit lower weight and greater durability under the variable-stator approach now selected than under the rejected dual-rotor approach.

The J-79 development proper began, then, about 6 or 7 months after construction of the research compressor began, and 1 or 2 months after work on the demonstrator engine began. The demonstrator program continued, with the original large engine scale, simultaneously with the development of the J-79. A separate team was assigned to it. It was not a "boiler plate" engine intended to demonstrate the previously tested research compressor (which was of approximately the same scale) with little attention paid to problems of weight and durability. Rather it was, in the words of a G. E. brochure, a "light-weight engine built as a flight type research vehicle." It was first run a year after it was begun in December 1953. Tests continued for a

number of months afterward. Its cost was a small proportion of the total development cost of the J-79.

At the same time that the demonstrator was being completed, the J-79 team was engaged in its own attack on weight and durability problems for the J-79 proper. This work involved building and testing components. It merged gradually into the accessory design, airframe matching, and complete flight engine construction work, which form the prototype development phase. The J-79 was first run in June 1954. After some detailed redesign work on compressor attachment arrangements (necessitated by an initial compressor failure), a 50-hour test was passed in August 1955, and a 150-hour qualification test was passed in December 1956.

The increase in knowledge ultimately due to the demonstrator dealt not with the performance made possible by a variable stator compressor (for this knowledge was obtained earlier through tests of the research compressor) but rather with the attainable level of weight and durability for a variable-stator engine of the demonstrator's scale. This knowledge implied (by application of "scaling" principles) some predictions about attainable weight and durability for a variable-stator engine of the J-79 scale. There is some evidence, however, that the brief period of work on the demonstrator had already yielded enough such predictions partially to support the final decision in favor of the variable-stator design. It is clear, on the other hand, that somewhat more accurate predictions were obtainable later, after completion and testing of the demonstrator.

Thus, in the case of the J-79, extensive testing of the design of what turned out to be the major critical component, and placing the test data on the shelf, made it possible to predict performance accurately after the general design study stage; hence the stage of performance-oriented development was short. The stage of weight- and durability-oriented development overlapped with (rather than succeeded) both the performance-oriented stage and the general design study stage, for the weight- and durability-oriented demonstrator was in fact begun when the design study was in progress. The

demonstrator's development overlapped, moreover, with prototype work on the J-79 itself. The demonstrator was built to a scale considerably larger than that of the J-79 itself and work on it was allowed to proceed only a short time before separate weight and durability work for the J-79 itself was begun. Had the demonstrator been close to the J-79 scale (which was specified in the general design study), and had its development not overlapped so extensively with the work of the team assigned to the J-79 proper, it might have better served to provide a sharp increase in predictability with respect to weight and durability. Nevertheless the largest uncertainties had been removed before any heavy commitment to the final J-79 design and scale was made and before heavy commitment to a using airframe. The J-79 reached the prototype phase relatively quickly considering the novelty of its design. It ultimately became one of the few engines to enter the commercial market.

THE J-57 AND J-75

The history of these two important Pratt and Whitney engines is very sketchily documented; critical decisions made within the development company are particularly hard to verify. The J-57, probably the most successful of all U.S. turbojet engines, was first contracted for by the Air Force in December 1948 after completion of a preliminary design study. It was an axial-flow engine obtaining considerably higher pressure ratios than previous U.S. engines; and it was the first U.S. engine to use a dual-rotor compressor.

A design close to that of the J-57 had apparently been incorporated, prior to the first contract, in a turboprop engine, the XT-45, which may or may not have run; the turboprop was canceled at the time of the J-57 contract. Moreover, there is evidence that prior to the first Air Force contract Pratt and Whitney conducted intensive component tests and completed and ran an engine that essentially was not only a performance-oriented, but also a weight- and durability-oriented demonstrator for the J-57. Hence, there is reason to believe that Pratt and Whitney could predict with high confidence that the levels

of performance, weight, and durability specified to the Air Force in the design study that preceded the contract, could be attained by an expenditure of the predicted amount over the predicted time period.

Nothing could be found in Air Force documents to indicate awareness of the existence of the demonstrator, and hence the Air Force could not have had the same high confidence as Pratt and Whitney. Nevertheless, the first large commitment to the J-57 -- the decision of late 1949 to use it in the B-52 bomber -- was made at about the same time as the signing of the first J-57 contract. Thus the Air Force may have thought it was making a large commitment in the face of major uncertainty but from the point of view of Pratt and Whitney (who knew the performance of the demonstrator that preceded the commitment) this was not the case. Other aircraft commitments were made shortly before and shortly after the first passing of a 50-hour test (August 1951) and during several years after this date.

The notably long list of military aircraft using the J-57 is included in Table 3 below. (In addition, the J-57 and J-75 have been the principal engines for commercial jet liners.) In none of these aircraft, it would appear, did major difficulties in the J-57 cause major delays or deficiencies.

It is worth noting here that Pratt and Whitney's own development expenditures (the initial pre-contract investigations probably constitute the bulk of them) seem to have played a large part in the success of the J-57. This success, in any case, appears to have established Pratt and Whitney as the leading engine firm, receiving the largest procurement orders and possessing the best reputation as a developer. The company's investment of its own resources seems to have yielded a return that justifies the investment.

The design of the J-75 was not quite as novel as that of the J-57: it had a different compressor, but it incorporated some features of the J-57. Nevertheless, Pratt and Whitney apparently were able to achieve the same high level of confidence, for the same kind of predictions, as in the case of the J-57. In this case, however, the Air

Force was apparently aware of most of the effort, for according to the 1953 History of Wright Development Center:

The contract had not been awarded to Pratt and Whitney until December 1952, yet by June the basic aerodynamic and mechanical design layout was complete, 95 per cent of the detail parts drawings had been completed and released for manufacture, and the remaining 5 per cent were drawings of internal parts which had a short manufacturing lead time. More than half of the parts for the first experimental engine were complete and the remainder were in the process of fabrication. An experimental J-57 had been modified to incorporate some of the unusual design features of the J-75 and instrumented to investigate some of the problems peculiar to that engine.

The high confidence turned out to be justified; for the J-75 exhibited closer conformity than any previous engine to the first official predictions of engine magnitudes. These official predictions were made only after the first major jumps in knowledge were past. Airframe commitments, moreover, were not made until these high-confidence predictions were available. The aircraft programs that used the J-75 experienced no important engine difficulties.

THE J-40

Since this case is very well documented,* we shall present its history in some detail. The J-40 was a Navy engine, but the difficulties experienced do not seem related to any difference between Navy and Air Force procedures.

On June 30, 1947, the Navy signed a contract with Westinghouse for the development of the J-40. Westinghouse was selected, according to Admiral Russell, head of the Navy's Bureau of Aeronautics, because "it offered to develop an engine with greater thrust or power than that proposed by any other concern and in a shorter time and at less

* As a result of Congressional Hearings. See Navy Jet Aircraft Procurement Program, Hearings before a Subcommittee of the Committee on Government Operations, House of Representatives, Eighty-fourth Cong., 1st Sess., October 24-27, 1955, U. S. Government Printing Office, 1956. The history given here is based on this document.

cost." In the original proposed design, the J-40 was an axial-flow single-spool engine weighing 3,500 pounds, developing 7,500 pounds maximum static thrust dry, 10,900 pounds with afterburner. No other engine in this thrust class was begun until 18 months later.

At the time of the contract signing, Westinghouse's reputation as an engine developer was very good. They had built the first American turbojet, the A-19. This had led to the first engine to pass a 150-hour test, the J-30, a generally successful engine. This had been followed by the J-34, a highly successful engine of which 4,500 had been built. It had performed well in Korea, powering the Banshee (McDonnell F2H) fighter.

After the first year of work on the J-40 program, Westinghouse had designed, still largely on paper, two families of engines. One had the thrust ratings of the original proposal and the other had considerably higher thrust (about 12,000 pounds maximum static thrust with afterburner). The higher-thrust engines, though of the same frame size (scale) as the lower-thrust group, were of substantially different design in their turbines and compressors.

Towards the end of 1948, after some 16 months of initial development work, Westinghouse received contract authorization to proceed with both the high-thrust engines (the J-40-10 was the principal high-thrust engine) and the low-thrust group (of which the J-40-8 was the chief representative). It is fairly clear that at the time this final authorization was made, the Navy had little more basis for prediction than a detailed design study with complete performance curves and Westinghouse's promise that a flyable J-40-8 engine would be delivered in 20 months and a flyable J-40-10 engine in 30 months.

In December 1948, the Navy held a design competition in the aircraft industry for a short-range interceptor. McDonnell was the winner of the competition with the F3H Demon using the low-thrust J-40. Although the Navy asserted that other engines could have been used, R. J. McDonnell testified, and the House investigating subcommittee

concluded, that no other existing or prospective engine could have been seriously considered at that time.*

In March 1951, the low-thrust engine passed a 50-hour test, only three or four months behind schedule. A prototype Demon, using the engine, flew in August 1951, with few engine troubles.

In January 1951, however, the Navy decided to change the mission of the Demon: it was now to be a medium-range, general-purpose, all-weather fighter. The major change in design which this required increased the weight of the plane from 22,000 to 29,000 pounds. The J-40-10 -- the high-thrust version of the J-40 -- was expected to be suitable for powering the redesigned plane.

In March 1951, the Navy placed a contract for 150 planes; 378 more were ordered in 1952 and 160 more in early 1953, making a total order of 688 planes in March 1953. With a J-40-10 engine yet to be run, the Navy let a contract to the Ford Motor Company for the production of 200 J-40-10 engines under license from Westinghouse. In December 1952, the order was increased to 800, and Westinghouse was given a production order for 700 of the J-40-10s. In addition, Ford was given \$50 million to build a government plant. Delivery of the engines was to begin in mid-1954. At the time of the Ford contracts there were orders still in effect for some 70 of the low-thrust engines to be produced by Westinghouse.

In July 1951, four months after the Ford contract, the first doubts about Westinghouse's promises appeared. McDonnell expressed fear that development of the J-40-10 was lagging far enough behind so that the projected aircraft delivery schedules could not be met. In the following months, the possibility of using engines other than the J-40 (in particular a modified J-46) was considered, but no action was taken.

* There is evidence that for several months after May 1948, serious consideration was given to the use of the J-40 in the B-52; the notion had been abandoned by October, however (with the J-57 selected). The Air Force also very seriously considered (and then abandoned) the possibility of using the J-40 in the B-66.

At the end of 1951, the Navy, now quite sure that development of J-40-10 was proceeding far slower than it should, authorized McDonnell to plan for interim installation of the low-thrust engines, some of which could be delivered in a year or so, in the first 12 Demons. By spring, 1952, prospects for the J-40-10 looked worse still, and McDonnell asked permission to install the low-thrust engines in 150 aircraft with retrofitting when the J-40-10 became available. The Navy agreed and, moreover, increased the order for the low-thrust engine by several hundred. At the same time McDonnell stated that aircraft so fitted would be disappointingly underpowered.

In the autumn of 1952, the Navy decided that the Allison J-71 of which an early version had just passed a 50-hour test, should be installed when available in some of the Demon airframes. The J-71 availability date was very uncertain, however, and no cancellation of any part of the Westinghouse or Ford commitment was made. McDonnell continued to plan for installation of the low-thrust J-40s in the Demon aircraft. The Navy was still hoping for ultimate success with the high-thrust J-40-10, and regarded the J-71 as equally uncertain (strangely, since it, at least, had passed a 50-hour test). The Navy, at this point, regarded the uncertain J-40-10 and the uncertain J-71 as insurance for each other.

Delivery of production models of the low-thrust engines began in November 1953. They were installed in a number of Demons. In March 1954, there began a series of 11 crashes of planes using the low-thrust engines, four of them with pilot fatalities. The low-powered Demons were reduced to the role of "limited life land-based development vehicle for the indoctrination and familiarization of pilots." Flight operations of these planes were canceled.

As for the high-thrust J-40-10 engines, the Navy finally abandoned these in September 1953, when the development program was terminated after engine development expenditures of some 40 million dollars. Another 40 million dollars had been spent for production tooling. Ford had built the 50 million dollar plant for the government. Westinghouse was paid 10 million dollars in termination costs and Ford 15 million dollars.

Far greater than the cost of the abandoned engine program, however, was the total development and production cost of the Demon airframe, estimated to have come to more than one-half billion dollars. What the Navy finally had to show for this expenditure is roughly this: 60 aircraft containing low-thrust engines not suitable for combat use; and some 200 airframes, some of which were eventually backfitted with J-71s at a conversion cost of about \$450,000 each. These aircraft, the only usable ones, were not available until well after the original intended date for operational Demons. They soon became obsolete.

In the J-40 case, it seems fair to say, the unfortunate consequences resulted from making a large engine-airframe commitment in the face of great technical uncertainty as well as uncertainty about the mission of the aircraft, on the basis of knowledge contained in an engine design study.

SOME SHORTER HISTORIES: J-46, J-33, J-35, T-35

The J-46

The history of the Westinghouse J-46, an engine begun in late 1949, is fragmentary. The fragments, however, give a rather clear impression of a "classic" case of engine-airframe commitments being made in the face of great uncertainty about all engine magnitudes.

The engine's design was new as were its performance goals. One important aircraft (both engine and airframe) was planned from the very beginning around the J-46. This was the experimental Douglas X-3 whose mission was to explore supersonic turbojet-powered flight. Two other fighter aircraft -- the Navy's Cutlass and the Lockheed XF-90 -- were later scheduled to use the engine.

The delays beyond the original schedule in attaining the performance, weight, and durability intended for the J-46 proved extreme. The highest powered model of the engine, which was to permit maximum information to be obtained from the X-3, receded so far into the

future that the incorporation of a much lower powered model became the aim. By August 1952, however, the lower powered model was 14 months behind schedule. In order to salvage any usefulness from the X-3, the plans to use the J-46 were canceled in late 1952. The X-3 flew with a much lower powered J-34 and never achieved supersonic speed.

As for the Navy's Cutlass, it finally received J-46s, lower powered than had been intended, and the program was greatly delayed and therefore greatly cut back. The Lockheed XF-90 was finally scrapped, largely for lack of a satisfactory engine.

The J-33 and J-35

These were the first U.S. turbojets to be produced in quantity. The initial proposals for both were made by General Electric in May 1943, and G. E. developed both simultaneously. The J-33 was first available for test in January 1944, and the J-35 in April 1944. The first 50-hour test was passed by the J-33 in June 1945, and by the J-35 in February 1946. The first 150-hour test of the J-33 (actually a later model) was passed in April 1947, and the first 150-hour test of the J-35 was passed in December 1947.

The J-33, a centrifugal-flow engine, did not involve (at the time of its inception) great unpredictability as to the difficulty of accomplishing the design study and performance-oriented development stages. There was considerable uncertainty, however, about the difficulty of achieving a durable engine, and a great deal of uncertainty about the difficulty of tooling and organizing for large-scale production of the J-33 (large-scale jet engine production had never been done before). Elaborate schedules for production and delivery of aircraft (principally Lockheed P-80s, intended originally for use in the war) were set up early in the J-33 development. They were based on equally elaborate schedules for production of J-33s.

It became evident in late 1945 that the planned J-33 schedules would not be attainable at G. E. and that the P-80 program would be

greatly delayed. In addition, durability, as measured by times between overhaul, promised to be considerably less than anticipated and hence the P-80 promised to be a less useful aircraft. The production difficulties were partially resolved by designating Allison as a secondary production source. In 1945, sole production responsibility was given to Allison.

Nevertheless, the failure of predictions as to engine durability and production schedule feasibility to be borne out by subsequent events led to an aircraft delivery schedule that lagged well behind, and an aircraft usefulness inferior to that originally assumed in making the choice of engine and airframe.

The J-35 was an axial-flow engine. In response to an Air Force proposal to finance development of a new engine (in addition to the J-33) and to let G. E. decide whether the design was to be taken from a British engine or to be "started from scratch," G. E. chose the latter course. The Air Force apparently recognized that there was considerable uncertainty about performance, and no specific aircraft commitments dependent on the engine were made until this uncertainty had greatly declined.

When commitments were made, however, in October 1944, there was still uncertainty as to the additional effort required to attain a producible engine and to prepare for large-scale production. The commitments required use of the engine in the Douglas XB-43, and the Republic XP-84 and P-84; they involved the ordering of 5,000 engines according to a detailed production schedule. The schedules proved impossible to attain, even though production sources in addition to G. E. were obtained, first Chevrolet, then Allison. (Allison, as in the case of the J-33, was given final responsibility for the engine, in 1948, to alleviate the pressure that several simultaneous engine programs imposed on G. E.)

The original schedules rested on the assumption that production tooling could be done while development of the engine was still far from complete, and that this tooling could remain usable. The assumption

proved false. Thus it was discovered, in spring of 1945, that production drawings that G. E. was scheduled to have prepared for Chevrolet some five months earlier were not ready. The delay in completion of these drawings, it was explained, "had been due to the amount of development difficulty encountered which had required detailed changes in design, so that the original drawings required extensive revision for production purposes."

In May 1945, the new knowledge about the difficulty of achieving large-scale production and a reexamination of the aircraft programs involved (in the light of the possibility of using J-33s instead of J-35s) led the Air Force to cancel 3,200 of the 5,000 J-35s which had been ordered. The planned P-84 program was cut down correspondingly, and a considerable delay in the scheduled delivery of the aircraft was accepted.

It may have been the J-33 and J-35 experiences that led to an attitude of caution in some Air Force quarters toward the next large engine-aircraft commitments that were expected to be made. The attitude was expressed by General Spatz in November 1945, when he said that the Army Air Forces "could well afford to delay allocation of funds for development of the new medium and heavy bombers in favor of maximum emphasis on development of new types of engines, because until engines with adequate performances are available, airplanes with desired characteristics cannot be built."

The T-35

The T-35 was a centrifugal-flow turboprop developed by Wright. The first design studies were completed (following an initial Air Force contract) at the beginning of 1945. The first intended application was to be to the B-36, but this idea was soon dropped. As the first designs for the B-52 were completed, the T-35 began to look attractive for this application. The original design of the T-35 was revised (a compressor stage was added) and greater power and lower specific fuel consumption were predicted for the final engine. In January 1947, a B-52 design study incorporating the T-35-3 (the revised

T-35) was submitted to the Air Force by Boeing and adopted (temporarily, as it turned out). Total accumulated work on the T-35-3 at this point consisted of the general design study and some detailed component drawings. No component construction had been begun. Uncertainty about all the engine magnitudes was considerable.

The result of the B-52 design adoption was that by the end of 1947, approximately 30 million dollars of Air Force funds had been committed to development of the T-35. In 1947, the T-35 (together with a much less extensive turboprop program -- the T-37 -- regarded as providing a "backup" for the T-35) absorbed about 60 per cent of the year's engine development budget.

Beginning in 1947, there were simultaneous development efforts to construct testable units of the T-35 turbine engine component (that is, the compressor combustion section and turbine) and of its propeller, driveshaft, gearbox, and power control components. The difficulties that arose in constructing the latter four components proved to be much greater than anticipated. As a result of this difficulty, and of information acquired about the turbine engine component, it began to appear that a combination of the turbine engine with a dual-rotation propeller instead of the single-rotation propeller originally planned would be preferable. This major change (which would require a coaxial shaft instead of the shaft so far developed) was decided upon, and a considerable delay in delivery of the first flyable engines for the B-52 was anticipated.

In February 1949, the intended B-52 design was changed; turbojets replaced the T-35-3 turboprops. The difficulties that had arisen in the T-35 program as revised to achieve the dual-rotation engine, and new information about the J-57 turbojet which promised to make possible the range required, were principally responsible for the change. The T-35 program was thereupon ordered terminated.

It seems clear that in the case of the T-35 there was a large commitment to engine development for a specific airframe when the uncertainties remaining with respect to the engine were still very

great. Changes in knowledge of other engines as well as information acquired about the T-35 led to a reevaluation of the commitment and a decision in favor of a turbojet. Sufficient information about the T-35 to justify a reevaluation might well have been attained, however, after a far smaller commitment than the one made, chiefly by inexpensive preliminary test work on propeller, gearbox, and driveshaft. This might have indicated, at much less cost, the advantages of, and an estimate of the difficulty of attaining, a dual-rotation engine.

TABULAR SUMMARIES OF UNCERTAINTIES IN THE ENGINE PROJECTS STUDIED

We shall now summarize some of the uncertainties that characterized a number of past engine projects. We want to stress our contention that these uncertainties are a natural and unavoidable property of engine development. The best predictions that can be made in the face of these uncertainties have a useful role in engine development strategy even though they may be very inaccurate predictions.

Table 3 deals with predictions made early in the development of 15 major engines (in most cases the predictions were made within a year of the award of the first development contract). They are predictions made by the Air Force (or Navy) as to the dates at which the 50-hour and 150-hour tests would be completed, as to the weight of the engine after the 150-hour test, and as to two of its performance parameters after the 150-hour test, namely "military thrust" and "specific fuel consumption at military thrust." Table 3 also shows the actually realized dates, weights, and values of the performance parameters after passing of the 150-hour test. In a few cases predictions and realizations for the first running of an experimental engine are also shown.

The prediction about the 50-hour test proved optimistic by one to three years in five cases and by at least six months in all but one case -- the sole case in which the actual date preceded the predicted date. The predicted date of passing of the 150-hour test proved early by one to three years in eight cases and by only a few months in the

Table 1

EARLY PREDICTIONS IN THE DEVELOPMENT OF J-3 ENGINES

Engine (turbojet)	Date of Predictions	Date of First Running, "Experimental"		Date of Passing of 50-hour Test		Date of Passing of 150-hour Test		Weight		Military Thrust/ESHP		SFC at Mil. Thrust/ ESHP (lbs/hr)		Maximum Thrust/ESHP ^b		SFC at Max. Thrust/ ESHP (lbs/hr)	
		Predicted	Actual	Predicted	Actual	Predicted	Actual	Pred.	Actual	Pred.	Actual	Pred.	Actual	Pred.	Actual	Pred.	Actual
J-40-1,-2	Mar. 1950	Oct. 1950	Oct. 1950	May 1950	June 1951	Sept. 1950	Feb. 1952	1,863	1,861	4,080	4,140	1.01	1.09	6,100	9,800	1.50	2.54
J-47-21 (later the J-73-3)	June 1951	Nov. 1951	Apr. 1952	June 1952	May 1953	Sept. 1952	Aug. 1953	3,528	3,880	9,100	8,929	.90 ^c	.917	8,100	8,100	.905	.917
J-48-5	June 1950	June 1950	June 1950	not test covered by J-48-1 test	Mar. 1951	Mar. 1951	none	none	none	6,350	6,350	1.16	1.17	10,900	11,000	1.50	2.35
J-53-1	June 1950	June 1951	Mar. 1951	Mar. 1951	Mar. 1951	Mar. 1951	none	6,500	8,000 ^c	17,500	15,150 ^d	.856 ^e	.938	17,500	?	.856	?
J-57-1	June 1950	June 1950	June 1950	June 1950	June 1950	June 1950	June 1950	4,300	4,200	9,250	9,400	.77 ^f	.795	9,250	10,000	.775	.795
J-57-3	Dec. 1950	Dec. 1950	Dec. 1950	Dec. 1950	Dec. 1950	Dec. 1950	Dec. 1950	4,348	4,250	6,700	8,700	.84	.84	8,700	8,700	.84	.84
J-65-1	Mar. 1951	Mar. 1951	Mar. 1951	Mar. 1951	Mar. 1951	Mar. 1951	Mar. 1951	2,600	2,695	7,220	7,220	.92	.92	7,220	7,220	.92	.92
J-65-1	Nov. 1951	Nov. 1951	Nov. 1951	Nov. 1951	Nov. 1951	Nov. 1951	Nov. 1951	5,000	5,103 ^g	13,200	13,800 ^h	?	?	21,500	21,500 ⁱ	2.1	?
J-67-3	Apr. 1953	July 1954	July 1954	Oct. 1954	Mar. 1954 ^j	Dec. 1955	Mar. 1959 ^k	6,904	7,603 ^l	12,950	14,050 ^m	.79 ⁿ	.855	22,350	23,000 ^o	2.35	2.40
J-71-3	Aug. 1950	Aug. 1950	Aug. 1950	Feb. 1952	Nov. 1952	Sept. 1952	Feb. 1954	4,350	4,350	9,700	9,700	.92	.92	9,700	9,700	.92	.92
J-75-1	Aug. 1952	Dec. 1953	Nov. 1953	Sept. 1954	Apr. 1954	Sept. 1954	Nov. 1954	6,200	6,100	15,500	15,500	.82	.82	23,500	23,500	?	2.03
T-34-3	July 1952	July 1952	July 1952	Apr. 1954	Apr. 1954	Apr. 1954	July 1954	2,500	2,590	11,000	11,000	1.440	3.085	11,000	11,000	3.560	3.685
T-47-1	Nov. 1951	Dec. 1951	Dec. 1951	July 1953	no test	Jan. 1954	May 1956 ^p	4,825	4,859 ^q	11,400	11,400 ^r	6.280	6.380 ^s	11,400	11,400 ^t	6.380	6.380 ^u
T-49-1	Aug. 1951	Aug. 1951	Aug. 1951	June 1952	Dec. 1954	Oct. 1952	no test	4,317	4,466	9,000	8,500	9.81 ^v	6.815	9,000	8,500	6.815	6.815
T-56-1	Sept. 1952	Sept. 1952	Sept. 1952	May 1953	Sept. 1953	Nov. 1953	Dec. 1953	1,510	1,595	3,507	3,507	2.04 ^w	2.075	3,507	3,507	2.046	2.025

Notes:

^aAt 150-hour test, or at last recorded date in engine's development, whichever is earlier.^bESHP at take-off.^cWADC History, July 1 to December 30, 1952, p. 298.^d"Historical Resume of XJ-33 Program," WADC Technical Report 53-467, December 1953.^ePrediction in April 1955. WADC History, January 1-June 30, 1955, p. 160.^fAccording to Aircraft Engine Characteristics Summary, 10 September 1950. "Phase 1 Preliminary Flight Rating Test Scheduled March 1951, Phase 2 Preliminary Flight Rating Test Scheduled March 1951."^gPrediction in Aircraft Engine Characteristics Summary, 10 September 1955.^hPrediction in Aircraft Engine Characteristics Summary, 21 December 1952. Funding subsequently stopped and no test occurred.ⁱPrediction in Aircraft Engine Characteristics Summary, 21 December 1952.

exceptional cases of the J-57 and J-75; a plausible explanation for the exceptions has been discussed above, as has a third exception, the J-79, not included in Table 3.

An uncritical glance at the predictions and realizations for weight, thrust, and specific fuel consumption might suggest that in many cases these predictions turned out to be accurate. But it must be recalled that a meaningful prediction as to these characteristics must also specify a time at which they will be attained. Thus the complete, meaningful prediction for each engine includes the predicted date for passing of the 150-hour test (after which the characteristics were observed) as well as the engine characteristics themselves. The total predictions made for each engine were, it is seen, generally far from accurate, and the extent of the inaccuracy varied widely.* It is this variation in the accuracy of predictions that suggests accurate predictions are very difficult to make -- that the size of the confidence regions R, discussed above, is small. That the error was almost always on the side of optimism is perhaps explained by the fact that contractors were the primary source of most of the predictions. Whether or not successive predictions for each engine were increasingly accurate -- as our general description of engine development suggests -- is not revealed in the table because records of successive predictions were not available.

No predictions were available for durability and production costs. If our model of engine development were correct, these would have been less accurate than the performance predictions. So far as weight

* A prediction for each engine as to the expenditure required to realize the predicted test dates, weights, and values of performance parameters was unavailable. Such a prediction must have been made for each engine if the total set of predictions was to be meaningful. It could conceivably be the case that it was predicted that at some expenditure the indicated predictions would be met, and that at some other expenditure (the expenditure actually incurred) -- the actually realized test dates, weights, and values of performance parameters might be attained. In this case Table 3 would falsely indicate inaccuracy of prediction. But there is not the slightest evidence to support this explanation of the discrepancies shown in the table.

predictions versus thrust predictions are concerned, the average of the prediction errors (when the errors are taken to be percentages of the true value) is less for the thrust predictions shown in the table than for the weight predictions (2.3 per cent versus 4.6 per cent).

Table 4 shows successive engines used in a number of aircraft. It is difficult to determine in many cases whether two or more successive members of the sequence of engines shown were planned to be successive engines in successive versions of the aircraft, or whether each successive member of the sequence arose because the predictions used in choosing the preceding engine turned out to be wrong. In either case, the same point is illustrated: it is extremely difficult to make a good choice of engine for a given aircraft when some of the engines to be considered are in early stages of development. If the whole sequence of engines shown for an aircraft was a planned sequence, because the "ultimate" engine was too far in the future and had to be preceded by others, than this illustrates that the choice may be extremely difficult, for it may sometimes have to be the choice of a sequence. If the sequence shown arose unintentionally, the original choice being the choice of a single engine, the difficulty is illustrated again. Very few aircraft end up with the engine chosen at the start of development.

Table 5 shows, for each of several major engines, the list of aircraft (many of them considered to be "successful" aircraft) that have used the engine. The list for some of the engines is very diverse. A "good" engine is used in many aircraft of different types regardless of its assignment to a particular aircraft at its inception; and the airframes that end up using such an engine are often developed quite independently of the engine, incorporating it only when its development is near completion.

SUMMARY

For an engine developed independently of an airframe the developer may constrain the performance, weight, and size of an engine

Table 4

SUCCESSIVE ENGINES PLANNED FOR USE IN VARIOUS AIRCRAFT

Aircraft	Engine Sequence ^a
<u>Air Force</u>	
F-84	J-65, J-73 ^b
F-86	J-35, J-65, J-47
F-89	J-35, J-71, J-65
F-100	J-57
F-101	J-57, J-67, J-75, J-79 ^c
F-102	J-33, J-40, J-67, J-57 (several models), J-75 (in F-102B, later called F-106)
F-104	J-65, J-79
F-105	J-71, J-67, J-57, J-75
F-107	J-57, J-67, J-75
B-47	J-35 (used in first two planes), J-47, J-57 (B-47C), T-49 (B-47D), T-47 (B-47D), J-65 (B-47D)
B-52	T-35 (several models), J-40 (seriously considered), J-57
B-57	J-65, J-57 (B-57D)
B-58	J-57 (originally planned for use in first 15 planes), J-79
B-66	J-40, J-71
SNARK	J-71, J-57
YC-130	T-56, T-38 (tentatively planned when delay of T-56 became apparent)
C-133	T-34, T-40 (tentatively planned when delay in T-34 threatened)
X-3	J-46, J-34
<u>Navy</u>	
F7U	J-34, J-46, J-35
F8U	J-57, J-75
F4D, F5D	Similar airframes, first used J-57, then J-79

(continued)

Table 4 (continued)

<u>Aircraft</u>	<u>Engine Sequence</u>
<u>Navy (continued)</u>	
F3H	J-40-10 (high-thrust model), J-40-8 (low-thrust model), J-71
F9F	J-42, J-48, J-33, J-65
F11F	J-65, J-79
XP6M	J-71, J-67 (seriously considered), J-75

Notes:

^aThese engines were either planned for use or actually were used by the Air Force and Navy for the designated aircraft. The engines in each sequence are listed according to approximate date of first decision for use in the indicated aircraft. No sequence extends beyond 1956.

^bThe J-65 was the intended (and is the current) engine for the F-84F. But in 1953 it was planned to fit two of the aircraft (to be called F-84Js) with J-73s "on the chance that the J-65s might have to be junked" (History of WADC, July 1-December 31, 1953, p. 165).

^cGeneral Electric proposed use of J-79-2s in the F-101A and received one F-101A from the Air Force for test installation of the engine.

Table 5

MAJOR ENGINES PLANNED FOR OR USED IN AIR FORCE AND NAVY AIRCRAFT^a

Engine	Air Force Aircraft	Navy Aircraft
J-33	F-80, T-33, XF-92, YB-61, YB-62, F-94 (A,B), TM-61 (tactical missile)	AJ2, F9F-7, TV-1, T2-V, P4M-1
J-34	X-3, XF-88	F3D, F2H, F6U, F7U
J-35	F-84 (B,C,D,E,G,H)	FJ-1
J-47	B-45, XB-51, XF-91 B-36, B-47, F-86 (D,F,K)	
J-48	F-94C	F9F
J-57	B-52, YB-60, F-100 F-102A, F-101 (A,B), SNARK, F-105A, F-107, KC-135A, B-57D, X-16	A3D, F4D, F8U
J-65	F-84F, B-57	F11F, A4D FJ-3, FJ-4, F9F
J-69	YQ-1, YQ-2, T-37	
J-71	SNARK, YF-89E, B-66	
J-75	F-101, F-102B, F-105 F-107	F8U, XP6M
J-79	B-58, F-104, F-101A (see note c, Table 1)	F5D, F11F, A3J, F4H
T-34	C-133A, YC-97J, YC-121F	R7V-2
T-40	XF-84H	R3Y, XFY, A2D
T-56	YC-130, YC-131C	

Note:

^a Aircraft in which engine was used or was planned to be used. For at least one (and generally more) of the aircraft in the list associated with a given engine, the decision to use the engine was made when the engine was in the final stages of development. (In the case of the J-57, J-79, and J-75 this is true of nearly all the aircraft listed.) No list extends beyond 1956.

at the start. The developer then heavily relies on the findings of the design study stage, even if these are unsupported by data on on-the-shelf components that are to be used in the engine. He decides, after that stage, on a single, highly specified design that will "best" meet the constraints. Assuming that the chosen paper design will be that of the final engine, the developer then tries, to some extent, to telescope the several stages of development. He also permits extensive simultaneous (rather than sequential) work on major components (compressor, turbine, and so on), even though the failure of one component to meet the predictions of the design study stage might require scrapping the work performed on another component.

In the case of an engine intended, at the start of development, for a specific airframe (which may itself just be starting development), the engine developer pursuing a strategy of the same type may display all of the above tendencies. In addition he is highly influenced from the start by the design of the proposed airframe. He makes his choice as to size and weight, for example, so as to fit the proposed airframe, as soon as some predictions about its relevant characteristics are available.

Alternatively a developer, whether or not a using airframe is specified, can avoid early commitments and can stress instead the quick attainment of some of the sharp jumps in knowledge that occur in engine development. A number of alternative designs may be explored, and the jump will serve to narrow the number considered. In the exceptional cases in which a very critical component of a proposed engine already exists, tested and on the shelf prior to the design study stage, the second type of developer may permit himself a somewhat closer reliance on the predictions. Such an exceptional case was the J-79.

IV. FIGHTER AIRCRAFT

(Based on a Study by L. L. Johnson)

THE F-100

Early Development History

North American Aviation was unsuccessful in the competition for a new interceptor held in 1950-1951. The winner was the F-102, to be discussed below. North American then, at its own expense, continued development of its unsuccessful proposal, an advanced version of the F-86, a fighter in satisfactory operational use.

The company spent about a year working on design studies and conducting wind-tunnel tests before receiving an Air Force contract for what was to become the F-100. Because of the desire of the Air Force for a new air-superiority fighter to combat the MIG-15 encountered in Korea, a decision was made in November 1951 to procure two prototype aircraft. Thirty million dollars were committed to the program, but only \$12 million were allocated in this initial phase. As a result of its earlier work, North American was able to provide a mock-up for inspection at the time of the Air Force decision to proceed with the F-100.

Negotiations leading to a letter contract were conducted during the rest of 1951. A clause was inserted calling for a production program (94 aircraft were mentioned), including the purchase of long lead-time items, spare parts, and tooling. The letter contract was signed in January 1952. Delivery of the two prototypes was scheduled for December 1953 and January 1954.

Amendments made soon thereafter radically increased the size of the program. Amendment No. 1, dated February 1952, called for 23 F-100A aircraft to be delivered from December 1953 through July 1954. Delivery of the prototypes was advanced six months, to June and July 1953. Amendment No. 4, dated 11 March 1952, authorized fabrication of tooling (jigs, dies, and fixtures) to support a production rate of 25 aircraft a month and capable of a peak rate of 175 a month.

Amendment No. 7, dated 26 August 1952, specified 250 additional vehicles to be delivered during the period August 1954 to July 1955.

The definitive fixed-price incentive contract covering these and other items was signed in December 1952. Some of the major items were:

<u>Item</u>	<u>Estimated Cost</u>
2 prototypes	\$ 13,579,950
23 aircraft at \$1,530,825	35,208,975
250 aircraft at \$299,426	74,856,500
Static-test article	1,530,825
Flight test	4,074,781
Spare parts	29,480,598
Spare parts	500,000
Engineering changes (allotted)	1,728,549
Other (wind-tunnel models, manuals, and so on)	<u>11,262,364</u>
Total	\$172,222,542

The prototype and production versions were to be identical except for the engine (the former had the XJ57-7; the latter, the J57-7), the afterburner nozzle, and the aft fairing of the fuselage underbody. Specifications of 18 May 1952 described a plane with a wing span of 36.58 feet, a length of 45.19 feet, and a gross takeoff weight (clean) of 24,989 pounds. Government-furnished equipment included a specified radar set, gunsight, and armament. Maximum speed was estimated at Mach 1.31, service ceiling at 55,700 feet, and combat radius at 505 nautical miles.

A major reason for the large commitment to North American prior to completion of a prototype was the feeling that the program did not entail a major advance in the state of the arts.

The selection of (this design) for production had been predicted on two principal circumstances: '...the confidence that our people have in the ability of North American to produce good equipment, and ...the fact that this airplane design does not represent major unknown areas of development.' The Air Materiel Command considered the airplane to be no more than '...a moderate advancement from the proven F-86 design.' Early production availability of a high performance air superiority fighter was the major

consideration in the decision to buy the 'Sabre 45' on an 'off the shelf' basis, 'without benefit of the usual experimental program.'*

On the other hand, there was some reason for believing that major unknown elements did exist in the program, for the plane had several novel features in wing construction, landing gear supports, and fuselage materials. In February 1954, moreover, North American commented in a report that "major development" would be required since the F-100 was to be the first combat aircraft capable of combat maneuverability and sustained flight at supersonic speeds.

The Air Force expressed considerable apprehension concerning the conduct of the program. The first production model was due only six months after delivery of the first prototype, a time plan that would not allow many changes in the aircraft following test-flight evaluations. In fact, the test-flight program as originally conceived was such that all the F-100s on contract would be delivered before flight testing could be completed. But the program was subsequently modified to include 36 aircraft in flight-test inventory to permit a more rapid completion of flight evaluation; nevertheless, the evaluation was to be completed only after a substantial number of aircraft had been produced. Moreover, the prototype itself was to be a production-engineered aircraft constructed with hard production tooling; any major changes found necessary during flight test could be time consuming and costly to incorporate on the production line because of the large tooling program planned by the time of first flight. In September 1952 (after the additional 250 aircraft had been programmed) Major General Albert Boyd stated that because several features of the F-100 would prove troublesome, a rapid production build-up should be delayed until evaluation of the first 25 aircraft had been completed. In the same week, Colonel V. R. Haugen, Chief of the Weapon Systems Division, warned that "...the early and rapid acceleration of this airplane into full production will cause considerable difficulty in

* Air Force Systems Command, Wright Air Development Center, Historical Resume' of the F-100 Program (n.d.).

reducing it to a practical, reliable weapon suitable for operational employment ...the schedule allows no opportunity for an orderly test program to uncover any unsatisfactory features which may well exist, before the production line is operating at full capacity."* In November 1952, General Partridge said, "I can only foresee that as now programmed, we are headed for another rash of groundings, retro-fittings ...and all the things that have plagued us recently in the B-47, F-94C and F-89 programs."** Neverthe'ess, the delivery schedule was not revised in any substantial way from that established in 1952.

The first YF-100A flew in May 1953, about 16 months after the initial implementation decision. Flight evaluation was completed four months later:

The test results indicate that the craft is superior in performance to any production fighter in the USAF. The most serious defects of the aircraft are the inadequate visibility over the nose during take-off and landing, the poor low-speed handling characteristics, and the negative to neutral static longitudinal stability experienced in level flight from approximately .8 Mach to maximum level flight speed.***

Flight testing continued into 1954 while North American proceeded with large-scale production. A follow-on letter contract for 230 aircraft in the C version was let in February 1954, and a definitive contract for 564 F-100Cs was signed in June. The first squadron delivery of the F-100A took place in September 1954.

During this period, stability problems, in particular, plagued the program. Late in the test-flight series in November 1954, a fatal crash caused by inertial coupling led to the grounding of the approximately 100 vehicles that had been produced during the 18-month span subsequent to first flight. To cope with these problems North

* Ibid.

** Ibid.

*** Air Force Flight Test Center, Phase II Flight Test of the North American YF-100A Airplane, p. ii.

American instituted a retrofit program on completed aircraft (consisting mainly of installing a larger vertical fin and adding a 12-inch extension to each wing tip) and incorporated modifications into the production line. The retrofit program was completed by August 1955. The first wing of F-100As was operationally equipped in June 1955, four and a half years after the development program had been started. Supplemental Agreement 43, signed in June 1955, provided for a cost increase on the contract of about \$7 million, which presumably covered the cost of the retrofit and modification program.

In mid-1955, two years after first flight, the F-100A underwent its operational suitability tests at Eglin Air Force Base. Even at this late date many problems were noted. For example:

Deficiencies in the engine limit the kill probability of the F-100A. These include compressor stalls with throttle manipulation and afterburner failure to ignite on many initial selections.*

. . . .

The F-100A is severely restricted from optimum combat performance because of compressor stalls. Experience has demonstrated that compressor stalls may occur at any combination of altitude, power setting and flight condition.... Once compressor stall commences, the pilot has little or no choice except to break off any attack and regain control of the engine by all means at his disposal.**

. . . .

In combat it will be difficult to tell if explosive projectiles are hitting the aircraft or compressor stalls are occurring.***

* Air Proving Ground Command, Final Report on Operation Suitability Test of the F-100A Aircraft, September 1955, p. 6.

** Ibid., p. 12.

*** Ibid., p. 35.

Serious limitations presently exist in the F-100A weapon system hindering its ability to deliver ordnance on an aerial target.*

. . . .

The results of an evaluation involving 29 sorties indicate that pilots flying the F-100A aircraft, as presently equipped with the MA-3 fire control system are not capable of firing satisfactory air-to-air gunnery scores. The exact cause of this limitation is unknown at this time, since on some passes with good tracking and with the pipper on the targets no hits were obtained. In addition, all pilots attempting air-to-air gunnery in the F-100A stated that smooth tracking was very difficult and that reticle vibration during gunfire was excessive.**

. . . .

When guns are fired at altitudes above 40,000 feet, an inverter failure indication sometimes occurs during the time of gunfire [this renders the gunsight and radar, as well as other equipment, inoperative]. Of even more significance, at altitudes above 50,000 feet a delay of up to five seconds often occurs between triggering and gunfire. The cause of these discrepancies has not been determined.***

These deficiencies were eliminated in subsequent development of the aircraft. A series of engine modifications and an intake duct redesign reduced the frequency of compressor stalling so that the operational capability of the aircraft was not seriously compromised. The fire-control system in the early aircraft was not modified satisfactorily. This system, which was developed during the time of the F-86, could not make adequate correction in the steering and target data presented to the pilot because of the flight characteristics of the aircraft, namely, a general lack of stability as a gun platform and a tendency to "porpoise" at certain speeds and altitudes. However, pilots well accustomed to the characteristics

* Ibid., p. 6.

** Ibid., p. 12.

*** Ibid., p. 13.

of the weapon system were able to achieve satisfactory target-practice scores by calling on their own judgment to aid in the operation of the fire-control system. For example, if they found that at given speeds and altitudes the system provided erroneous information regarding the amount of target lead, they could learn to make allowance for it.

In the spring of 1955, 70 aircraft of the 250 that had been ordered under Amendment 7 were transferred to another contract covering production of the C version of the F-100. The initial contract, the only one under which the F-100A was procured, therefore covered the two prototypes and 203 production vehicles. The last aircraft on the initial contract was delivered in the summer of 1955.

During the time these aircraft were produced there were many other changes and amendments to the contract that generated both cost increases and decreases. For example, a reduction totalling about \$8 million was occasioned by a decrease in spare parts procurement. Small cost increases occurred because of numerous minor modifications, procurement of 500 wing tanks (\$740,000), and a second mobile training unit (\$495,000).

A firm (reset) target price for the 206 airframes (two prototypes, 203 production version aircraft, and one static test article) was established in March 1955 for a total of nearly \$136 million. This figure corresponds to a \$103 million figure calculated according to the terms of the original contract -- not a wide discrepancy by comparison with other projects. In March 1957 a final renegotiation took place on this contract. The final price of the 206 aircraft came to about \$134 million. With the renegotiated prices for the rest of the items covered in the contract, the over-all total (including fee) was about \$186 million.

Besides the 205 F-100As, over 2000 F-100s in the C, D, and F versions were ordered by the Air Force. Over 1000 were delivered by the end of 1956, with little slippage from original schedules. The C, D, and F differed from the A primarily in modifications for larger

external stores in fighter-bomber roles, and alteration in subsystems. The F-100D, for example, could carry either a number of rockets in six underwing clusters for a primary mission as a fighter bomber or four Sidewinder missiles as a day fighter, in addition to its guns.*

Remarks

Several points in the F-100 history are important. First, it is notable that the aircraft, though originally designed as an air-superiority fighter, apparently proved quite versatile, for it was adaptable to a fighter-bomber role. North American was able to add various combinations of off-the-shelf items to the basic airframe and roll out a weapon system that the Air Force bought in large numbers. Although only about 200 aircraft were procured in the version originally planned, and although there were a number of difficulties with this particular version, ten times that number were procured in the subsequent versions. This illustrates two kinds of uncertainty: (1) components often turn out adaptable to weapon systems for which they are not originally designed, and (2) the preference scale of the Air Force may change through time. Even if the Air Force gets the system it originally ordered more or less as planned, it may find another system preferable. While the whole F-100 program was begun on the premise that an advanced air-superiority fighter was needed, the Air Force, apparently finding a few years later that a fighter-bomber was more badly needed, procured ten times as many fighter-bombers.

Second, although great caution is necessary in comparing one aircraft with another (there are features that are simply not comparable), it is notable that the development time of the F-100 program was less than that of any other of the "Century Series" (which included the F-101, F-102, F-104, F-105, F-106). Even allowing for the year

* The addition of the Sidewinder missile to the F-100D system gives the aircraft a satisfactory capability as an interceptor within certain environments.

spent by North American on its own, the first wing was operationally equipped four and one-half years after initiation of the program. Time spent from USAF implementation to first squadron delivery for other Century Series aircraft was in no case less than five years.

Third, the cost of development through the initial contract was relatively low. Of the other Century Series fighters only the F-104A appears to have been equally inexpensive.

Finally, development of the F-100 proceeded smoothly. Aside from the inertial coupling problem, the airframe showed no serious aerodynamic deficiencies and there were no significant delivery slippages. The design proposed in 1951 was developed without major change. The F-100 is the only one of the Century Series in which the engine originally selected appeared on the finished plane. Cost overruns were modest in comparison with those found in some of the other Century aircraft; and major performance parameters of the finished product were not far different, except for range and ceiling, from those predicted by North American at the time the definitive contract was signed.

How can the relatively short lead time, low cost, and reasonably straightforward character of the program be explained? While no complete answer is possible, there appear to be three factors of particular relevance. First, the design was based roughly on the configuration of the F-86, an aircraft that was well proven and by any reasonable standard quite successful. At the low supersonic speed required, and for the tactical roles planned for the F-100, the basic design simply did not exhibit major deficiencies. There were no unexpected transonic drag rises, pitch-up problems, or unexpected weight increases, characteristic of certain other programs, that might have degraded performance. In short, there was a wide range of configurations -- delta wing, straight wing, swept wing, single-engine, twin-engine, nose air intake, side air intake, and so on -- from which to choose in designing an airplane with the general capability of the F-100, and some would in all probability have been

better than others. North American chose one in which the uncertainties were known to be relatively few because of its close relation to the F-86.

Second, the airplane was based on off-the-shelf procurement of proven major components other than the airframe. The electronics (including the radars) and gunsight had been developed earlier and had already seen operational use. This procedure helped the aircraft program in two ways: (1) no major problems arose concerning availability of these components when they were needed on the production line; (2) the principal area of uncertainty was confined to the airframe-engine combination. Such is not the case when all major components are designed at the paper stage to work together as a weapon system before they are tested in hardware form. In such situations, if unforeseen problems occur in the development of any one component they can have a serious effect on the development of the whole system. It should be pointed out, however, that despite the fact that the subsystems did not involve any major development problems, problems did arise in getting them to work properly after the airplane had been put into operational use. As for the engine, it is true that the J-57 was still in development when North American selected it. However, it had passed its 50-hour test in August 1951, three months prior to Air Force implementation of North American's proposal, and it had already powered the B-52 prototype, which made its first flight in April 1951. Although this selection did not preclude compressor stall and afterburner deficiencies, North American did get an engine that in general met specifications, was relatively reliable in operation, and was available more or less on schedule.

Third, and perhaps most important, it is notable that the Air Force had a better basis for committing itself in the F-100 program, in terms of quantity of information available to it, than it did in other Century Series developments. It had the benefit of the information gained by North American during the year the company worked alone: the mock-up inspection was held at about the time the Air Force gave the initial go-ahead. In other Century programs, mock-up

inspection was held some time after initial commitment. The importance of this observation lies not in the fact that mock-up itself is of primary importance, but in the fact that typically many of the changes and modifications occurring in aircraft programs are made on the basis of wind-tunnel tests, rocket tests, and paper calculations performed prior to mock-up. By the time mock-up is completed, the specifications of the aircraft provide a more reliable basis for Air Force commitment than those drawn up earlier. In the case of the F-100, the Air Force's initial "bet" was a heavy one but it was placed late in development compared with other fighter programs.

THE F-102

Development History

In the late 1940s several factors were of particular importance in shaping the concept of air defense out of which the F-102 emerged. First, the interceptors soon to enter inventory were expected to have only marginal capability against Russian jet bombers expected to become operational in the early 1950s. Flying at near Mach 1 speeds at altitudes of around 50,000 feet, these bombers would provide elusive targets for the existing interceptors -- the North American F-86D and the Northrop F-89, which were subsonic and had combat ceilings of under 50,000 feet.

Second, successful interception of such bombers involved reliance on the continued development of air-to-air missiles, rockets, and radar fire-control systems to provide all-weather capability in detecting and identifying enemy aircraft, in positioning the interceptor in the optimal flight path for the kill, and in preparing and firing weapons at the proper instant of time. In the new era of extreme speed, visual methods of aiming and conventional machine gun armament became hopelessly obsolete. At the same time, the increasing complexity of electronic equipment threatened to make impossible demands on the single pilot during the final intercept phase. Use of a second crewman, as in the F-89, to ease this burden involved

weight and space penalties that made this solution only a stop-gap measure. Making the "ultimate" fire-control system as automatic as possible was therefore stressed.

Finally, in the late 1940s a strong body of opinion emerged in favor of designing airframe, engines, fire control, and other equipment to be operated together from the outset in an integrated weapon system. This approach stood in contrast to the more or less piecemeal fashion in which technological advances (such as the B-29, P-51, and F-80) of World War II and the early postwar years had been made. Typically, under the latter approach an airframe was developed, after which the engines and supporting equipment, developed under separate programs, were requisitioned off the shelf to complete the system. Problems arose because components so tied together were not always compatible: an airframe might be developed for which no suitable bombing-navigation system was simultaneously available, or environmental conditions imposed by the aircraft might be outside the tolerance levels of its electronic equipment. Because it was felt that the attainment of ever higher levels of aircraft performance in the future would entail progressively more restrictive environmental conditions, more stringent space limitations, and greater difficulty in keeping component development in phase, the notion of applying the new "weapon system concept" to subsequent aircraft development became increasingly popular.

In 1949 Air Force Headquarters accepted the idea of a coordinated development program of an interceptor system capable of dealing with the enemy threat during the 1954-1958 time period. A directive to the Air Materiel Command (AMC) called for "an interceptor competition ...to meet military characteristics now awaiting approval."

In May 1949, Air Force officials held a series of presentations in Washington before industry and military representatives. Among other things, they outlined the problem of air defense and broached the subject of the weapon-system concept in which they envisaged the various components such as airframe, armament, ground and airborne

radar, communications, servicing facilities, and so on, as forming an integrated, complete defense network.

Since the Air Force believed that development of a fire-control system would take longer than development of the airframe, and since the airframe was planned to be tailored to the requirements of the electronic and control system, a competition for the electronic and control system was held prior to the competition for the airframe. Requests for proposals and an outline of requirements for the electronic and control system were sent to 50 firms in early 1950. By early April, 18 companies had offered proposals. Cost estimates for the development program ranged from \$1,680,000 to \$14,250,000. In July 1950, Hughes was named winner of the competition, and a first-year contract was subsequently negotiated for its MX-1179 electronic and control system.

In September 1950, AMC sent requests for airframe proposals to 19 aircraft companies. Requirements were written around an interceptor to be operational in the 1955-1959 time period and capable of intercepting bombers that have a maximum speed of Mach 1.3 and fly at altitudes up to 60,000 feet. Armament and combat radius were specified. The aircraft was to be directed automatically to the target area by the ground-based aircraft-warning and control system tied directly by data link to automatic intercept-course computers and an automatic flight-control system in the aircraft. After the aircraft locked onto the target, the armament was to be aimed and fired as directed by the fire-control system. It was planned that the pilot have only a monitoring function during the intercept. That the new weapon-system concept was to govern the development of the interceptor was emphasized in an introductory statement to the request for proposals:

The problem of interception can be solved successfully only by effecting the highest degree of integration of electronic and control equipment into the design of the airplane. To insure the success of the new interceptor, it will be mandatory for the aircraft and the electronic and control system manufacturers to coordinate extensively both developments.

In this respect, the prime responsibility for the satisfactory functioning of the airplane as a weapon will rest with the aircraft manufacturer.

By the end of January 1951, the deadline for replies, six firms had submitted a total of nine proposals. Republic offered three, North American two, and Chance-Vought, Convair, Douglas, and Lockheed each proposed one design.

For our purpose, we need discuss in detail only the Convair proposal for the aircraft that became the F-102. It called for a delta-wing, single vertical-fin configuration. The advantages claimed for this configuration were:

1. Low weight and high rigidity with very thin wing sections.
2. Low drag at transonic and supersonic speeds.
3. Adequate stability and control without addition of a horizontal tail.
4. High maneuverability and freedom from buffeting with a smooth stall development and excellent spin recovery characteristics as compared with conventional swept-wing configurations.

Convair specified the Wright J-67, an engine presumed to be available by June 1954, for use in the ultimate version. In addition, it suggested the Westinghouse J-40, programmed to be available nearly 3 years earlier, for use in early test-flight vehicles. A prototype, using the J-40, was expected to fly in 1952 or early 1953. Intended specifications for each version of the aircraft are shown in Table 6.

In each version the first three estimated specifications were a substantial improvement over the minimum performance requested by the Air Force. Convair provided cost estimates for two prototype airframes, for the first 25 production airframes, and for the first 300 production airframes. Convair further predicted that given an order for 300 units, a March 1951 go-ahead, and availability of Hughes' MX-1179 (the Air Force specified fire-control system) in June 1953, deliveries would start in 1954 and a production peak of 15 per month would be reached in the spring of 1955.

Table 6

SPECIFICATIONS FOR TWO VERSIONS OF THE F-102

<u>J-40 Engine</u>		<u>J-67 Engine</u>
(13,700 pound thrust with afterburner)		(21,100 pound thrust with afterburner)
Maximum speed (Mach)	1.88	1.93
Combat ceiling (ft)	56,500	60,200
Combat radius (n.mi.)	715	768
Take-off weight (lbs)	22,472	22,940

On the basis of these predictions and similar predictions for the other interceptor proposals in the contest, Air Force Headquarters announced in July 1951 that Convair, Lockheed, and Republic were the winners of the competition, that each would be given a Phase I contract, and that one would be given a production contract on the basis of the results of Phase I work. Phase I comprises "preliminary design and mock-up." (Succeeding phases, which have sometimes been separately contracted for, are Phase II, "fabrication"; Phase III, "preliminary flight tests"; and Phase IV, "final performance tests".) It appears, therefore, that the intention of the Air Force was to keep several contractors in the program but to make only small commitments to each of them early in development in order to "purchase" information and delay the major decision concerning a production contract, because of the relatively high costs involved, until more knowledge was available. But the decision to sponsor closely competing programs was soon revoked. Only a month later Lockheed was notified that it would not be given a development contract. In addition, Republic was informed that development of its aircraft would continue as a separate, long-range program because its success would rest on development of the J-67 in combination with a novel ram-jet arrangement utilizing the afterburner, a joint development that would take some years. Consequently, by the end of September 1951, Convair remained the only contractor upon which the Air Force could depend for development and production of an interceptor whose characteristics might have been expected to meet the requirements set forth in the competition. The original commitment to Convair, however, remained quite small in dollar terms until the fall of 1951.

When the pressure of events in Korea mounted, Air Force Headquarters concluded that none of the proposals would result in an operational aircraft by 1954. It appeared that Convair's J-67 version would not be available before 1956 because of expected delays in delivery of the fire-control system and the engine. An obvious candidate for an "interim" interceptor for the 1954-1956 time period was the J-40 version; this could at least serve in an extensive

flight-test program. Cost estimates were requested in October from Convair covering development through two prototypes and production of 25 airframes in the first year, with tooling to support a buildup to 50 per month. Other candidates for the interim role, from other contractors, were also considered.

In November 1951 the Air Force decided in favor of Convair's J-40 version, to be used in an accelerated test-flight program, involving 50 aircraft. The J-57, however, was now substituted for the J-40. These 50 aircraft were designated the YF-102; the follow-on version of the interim aircraft was designated the F-102. (The "ultimate" version of the aircraft was designated the F-102B in late 1952.)

Production was to be conducted under the so-called "Cook-Craigie" plan in which Convair was given the green light to proceed with production tooling prior to first flight of the prototype. While Convair was eliminating troubles in the airframe in accordance with flight-test results, it could simultaneously be modifying its tooling, and presumably be in a position to move into large-scale production buildup within a relatively short time.

This was a major decision involving expenditure of many millions of dollars for tooling, manufacture of hardware, and flight testing, in contrast to the hundreds of thousands of dollars involved in the Phase I contracts planned at the time of the aircraft competition. The Air Force did not immediately award a definitive contract to cover this additional work. However, in January 1952 Convair's original letter contract was increased by several million dollars to start a production-engineering and tooling program. No large-scale production schedule was set at that time, but in early 1952 Convair was authorized to proceed with two YF-102 prototypes to be delivered in June and September 1953 and seven production-version aircraft to be delivered from January through August 1954.

There were several factors that apparently motivated the Air Force to make this major commitment early in the development of the

aircraft. First, there was a growing preoccupation in the Air Force with reducing "lead-time" consumed in moving from paper-stage planning to operationally suitable aircraft. Under the development approach previously followed, typically one or two prototypes were "hand-made" with relatively crude and inexpensive tooling, and the procurement decision postponed until after flight-test data were available. That this procedure might involve excessive time and duplication is well expressed in a May 1952 statement:

It is not believed that an airplane produced partially by hand and partially with temporary tooling is a true representation of the final production article. As a result, much of the flight testing may have to be repeated on the first airplane fabricated with production-type tooling. Also, the fact that some parts can be satisfactorily fabricated by hand does not always mean that some parts can be readily produced within the acceptable tolerances with production tooling. ...Should the results of the first six months flight testing be favorable and a decision made to produce the aircraft in quantity, the time consumed in manufacturing the first article with production tooling would be essentially the same as for producing an entirely new aircraft.*

In addition, a rapid production buildup on the basis of limited flight testing of nonproduction prototypes had in the past frequently resulted in major defects being uncovered only after a large number of production aircraft had been built. (One of the best examples here is the B-47 experience, discussed below.) This alteration resulted in expensive and time-consuming retrofit programs.

A proposed solution to these problems, the Cook-Craigie plan, enjoyed increasing Air Force support in the early 1950s, and the F-102 became the first aircraft to follow this plan. By constructing production-engineered aircraft with production tooling at the outset and holding output to a low level for about 18 months during the test program, the manufacturer would presumably discover and

* Letter from Air Material Command to Headquarters, USAF, 16 May 1952, subject "Initial Production Rate for New Model Aircraft."

remedy the major difficulties before undertaking large-scale production. Furthermore, it was hoped that time and money would be saved in moving from an experimentally designed article to one suitable for mass production; that development, production engineering, and tooling cost of the interim version of the interceptor would not have to be duplicated for the ultimate version, since the original plan was to construct a common airframe for both. Only the engine and possibly the fire-control system and other supporting equipment were to be different. Consequently, the Air Force had no reason to believe that costs for the over-all program would significantly increase because of the independent interim effort. The "best guess," moreover, was that the MX-1179, the designated fire-control system, would be available for the interim aircraft and that no separate interim system, which would involve additional development cost and possibly delays in the MX-1179 program, would be necessary. So far as engines were concerned, the J-67 remained programmed for the ultimate version, to follow the J-57-powered interim version.

It is notable that the Air Force had only limited knowledge concerning the performance and cost of the interim aircraft at the time it authorized the interim program in November 1951. It had the cost estimates provided by Convair in October; it also had Convair's estimate of performance with the J-57, which included a maximum speed of Mach 1.5 and an altitude of nearly 60,000 feet. But neither the aircraft laboratory at Wright Field nor the National Advisory Council for Aeronautics (NACA) had yet conducted wind-tunnel tests to verify Convair's figures.

During 1952 and early 1953 several things happened to bring about a radical shift in fire-control system programming. Hughes was beset with continuing delays, and the MX-1179 was falling behind schedule. In addition, during the summer of 1952 the Air Force canceled the F-89F interceptor program, the aircraft that had been intended to precede the interim F-102 in the inventory of the Air Defense Command. This decision made it even more imperative that the over-all interim program remain on schedule, which in turn forced

a reappraisal of the availability and capability of alternative fire-control systems. The best bet for an interim system appeared to be the Hughes E-9, originally programmed for advanced F-89 interceptors, appropriately modified for use in the YF-102. Hughes estimated that a crude system would be available for installation in an early YF-102 by July 1954 and a refined version available in production quantities two years later.

A decision regarding the E-9 presented the Air Force with a dilemma. If no interim fire-control system program were interjected, it was felt that the MX-1179 would be ready for testing by December 1954, in phase with the YF-102 test flight program, and that production quantities would be available by April 1957, about a year behind the schedule for the E-9. However, with an interim fire-control system in the picture, the MX-1179 might be delayed an additional 18 to 24 months. Moreover, the E-9 as then developed had few of the automatic features of the MX-1179. The pilot would have to fly manually to and from the target area as directed by verbal ground instructions; he would not have the benefit of automatic flight itself, and there was no provision for an autopilot, which was considered essential for long intercept flights.

In early 1953, Headquarters USAF decided in favor of the E-9 and shortly thereafter approved a proposal to develop an E-9 autopilot to be available for retrofit by September 1956. In addition, the decision included initiating work on a program involving pilot-assist subsystems aimed at providing automatic flight control and automatic attack modes. Possibly because of the increasing cleavage between the interim and ultimate versions of the aircraft, the nomenclature was revised: the interim version was redesignated the F-102A, while the ultimate version was designated the F-102B. In the words of the Air Research and Development Command in March 1953:

Every effort should be exerted to expedite the availability of the F-102A whose configuration has been determined as incorporating the E-9 Fire Control System and the J-57 engine. The F-102B configuration with the MX-1179 and J-67 engine will be phased in at the earliest date without affecting E-9 availability for the F-102A.

An important point concerning the E-9 is that much more than a repackaging job was involved in the initial program. About half of the equipment had to be designed specifically for the F-102A in order for the system to operate at altitudes up to the combat ceiling of the aircraft. The original E-9 had been developed for lower altitudes. Because of the major changes involved, the fire-control system for the F-102A was redesignated the MG-3.

During 1952 and early 1953, major changes were also made in the airframe involving substantial weight penalties, which were later to play a critical role in the performance of the airplane. As originally designed, the aircraft was to carry its air-to-air missiles in a bay directly below the engine and the rockets in a forward bay. This arrangement was based on a missile wingspan of 20 inches. Later the developer of the designated missile discovered that the missile would have to be enlarged, and, in particular, its wingspan would have to be increased. The corresponding increase in the size of the missile bay and the resulting increase in fuselage diameter would substantially reduce the top speed of the F-102A; therefore, a complete redesign of the armament bays was necessary. The missiles were installed in two tandem bays and the rockets placed in the doors of each bay. The fuselage was lengthened and with resulting changes in controls, wiring, and so on, the airframe weight increased. There was a roughly equal increase in missile weight. Changes in armament and additional airframe changes led to a still further weight increase. The total weight increase was substantial.

Late in 1952 the Air Force negotiated a definitive contract (cost plus fixed-fee) for nearly \$100 million covering production of the 42 aircraft to be delivered in 1954 and 1955. In 1952 and 1953, there was a debate about the drag of the F-102A that culminated in an extensive modification of the aircraft. Early in 1952 Wright Field engineers disputed Convair's prediction as to ceiling and combat range for the J-57 version, believing that insufficient allowance had been made for "trim drag." NACA subsequently conducted an analysis of drag and came to conclusions that were disquieting: actual ceiling

was estimated at 5,000 feet less and combat range at one-third less than the Convair prediction. Even the supersonic capability of the aircraft was held in doubt, all because of an expected "unusually high transonic drag rise." NACA, furthermore, had developed the "ideal body theory" and recommended that it be incorporated in the F-102 design. Very briefly, this theory, based on the work conducted in 1952 by Richard Whitcomb and R. T. Jones, indicated that in order to compensate for the drag of a delta wing at transonic and supersonic speed, the fuselage would have to be indented at the juncture of fuselage and wing, and elongated to conform to a minimum acceptable ratio of fuselage length to cross-section area.* By early 1953, wind-tunnel and rocket tests of models incorporating the indented and elongated fuselage confirmed the belief that Convair's early estimates were wrong. In August 1953 Convair accepted the "ideal body theory" and joined in recommending the appropriate modifications.

These modifications were fairly extensive. Besides the indentation, they involved lengthening the fuselage by seven feet and moving the wing and vertical fin rearward. In addition, a cambered leading edge to increase lift and "warped" wing tips to reduce trim drag were included in the program. The modifications amounted to a 1100-pound weight increase. New specifications for the F-102A were issued. They called for a lower ceiling at combat speed than the previous specifications.

The configuration change complicated the program because under the Cook-Craigie plan Convair had already tooled up for production of the old configuration. Changes in tooling would involve scrapping about two-thirds of the tools already procured. Because hardware fabrication was well along for the first few aircraft, Air Force Headquarters authorized that the two prototypes (having the old configuration), be delivered in October and December 1953 as scheduled,

* The "Coke-bottle" modification was intended to improve performance in the transonic region, and a different modification, later developed by Jones, was intended for aircraft flying in the Mach 1.2 to 2.0 regions.

that an additional eight of the old configuration, already far along in development, be completed during 1954, and that beginning with the eleventh aircraft all the remaining 32 incorporate the modifications. The first ten were designated YF-102, and the new version, F-102A. (The "ultimate" version, the version proposed originally by Convair with the J-67 engine and the MX-1179 fire-control system, remained the F-102B.)

The prototypes, built with production tooling, were delivered on schedule. Flight tests confirmed the fear that the plane would be subsonic. Maximum altitude tests in April 1954 indicated a combat ceiling below 50,000 feet, well under that originally intended.

At the beginning of 1954 it became apparent that more than the "ideal body" modification would be necessary to provide adequate performance in air defense. By itself the modification was expected to add only .1 Mach to maximum speed (with the J-57 engine), while combat altitude would remain below 50,000 feet. The F-102A had simply grown too much in weight since it had been originally conceived in 1951. Take-off weight with all the preceding modifications, and many others, had risen to about a third more than the original estimate. Air Force officials felt that only an engine such as the J-67, in the 20,000 pound thrust class, as compared with the J-57's 15,000 pound thrust, could provide the kind of performance needed. But prospects for the availability of the J-67 were growing bleaker and bleaker. In fact, there was increasing talk of substituting the J-75 for the J-67 in the "ultimate" version (F-102B). Rather than halt development pending availability of a larger engine, it was decided to make a drastic reduction in the weight of the airframe of the F-102A. The airframe had been designed to withstand the stresses that would be exerted in flight with an engine producing over 20,000 pounds thrust. With the 15,000 pound thrust J-57, it would be possible to reduce the dimensions of structural members without reducing structural integrity within a lower-stress flight environment, but this involved serious problems. First, the airframes originally planned for the F-102A and

the F-102B were to be identical, thereby requiring only one set of development and tooling costs. With two distinct airframes in the program, new F-102B airframe development and tooling costs would have to be added to all the costs incurred in the interim program. Furthermore, the tooling needed to produce "ideal body" F-102As beginning in early 1955 was modified from that employed in constructing the ten YF-102s. To carry out a second program involving weight reduction, even more tools would have to be replaced or modified and substantially more engineering changes would be needed.

The Air Force decided that the second modification program held sufficient promise of salvaging the F-102A so that it was worth the expected cost. The program involved not only reducing weight by about 2400 pounds, but also modifications involving nose lengthening, canopy redesign, intake redesign, and aft wing fillets. Even so, the planned take-off gross weight of the final lightweight version was still about one-fourth more than the original estimate made in 1951. Rather than produce the remaining 32 aircraft under the contract with only the "ideal body" modification, Convair was not directed to apply both modifications to 28 of the aircraft. Only these last 28, therefore, were expected to enter the air defense inventory. In summary, the following is a tabulation of the models comprising the 42 aircraft:

	2	production prototypes (YF-102)
	8	unmodified production versions (YF-102)
	4	"ideal body" modification, heavyweight versions
	28	both "ideal body" and weight reduction modifications
Total	<u>42</u>	

Convair produced the remaining eight of the first (unmodified) version during 1954. These were used in various phases of flight testing that did not involve supersonic flight. The first "ideal body" aircraft flew in December 1954. In order to approximate the flight characteristics of the later, lightweight aircraft, it underwent an arbitrary weight-reduction program in which over a ton of

equipment was eliminated. Flight tests with this aircraft indicated substantial improvement in speed and an altitude capability superior to that of the F-102. After discarding about one-half of the tools used to manufacture the four heavyweight F-102As and adding a somewhat greater number of tools to the production line, Convair completed the first lightweight version in June 1955. The 28 lightweight aircraft were produced at the rate of 2 to 3 a month; the last one was delivered in March 1956. All of these, it will be recalled, were to be used as test-flight vehicles.

Phase IV performance tests on the F-102A (lightweight, "ideal body") were conducted at Edwards in early 1956. The evaluation, which for the most part covered testing of the airframe-engine combination (not the complete weapons system), was very favorable. The aircraft was pronounced "greatly superior" to any all-weather interceptor then in use.

The total cost of attaining these 42 aircraft, excluding the flight-test costs, turned out to be about double the original allotment for this purpose. Some of the additions involved the two major modification programs; others involved new items.

It turned out in 1956 that the complete weapon system, including missiles and fire control system, was unsatisfactory. The following two years saw an extensive program of modifying the troublesome components and retrofitting the aircraft. When the program was completed in late 1958, the F-102A weapon system attained an acceptable level of over-all effectiveness.

As for the "ultimate" version of the interceptor, the version with which Convair had won the competition back in 1951, a contract was let in 1955 for a number of these (redesignated the F-106). An engine switch from the J-67 to the J-75 was made in 1955, the original fire-control system remained. Numerous modifications, delays, and improvements, not foreseen at the signing of this contract, followed. Budgetary constraints and the success of competing fighter programs led eventually to a substantial cut in the number of these aircraft

finally procured. Their maximum speed and combat ceiling fell short of the original 1951 specifications (for the J-67 version).

Remarks

The F-102 and F-106 programs had two outstanding and related properties:

(1) A complex "system," made up of many components, was highly specified from the start and heavy commitments were made quite early on the assumption that the initial performance predictions would be met in the specified time interval.

(2) An attempt was made (the Cook-Craigie Plan) to telescope two of the most time-consuming steps (testing and production tooling) in the task of attaining the required number of operating aircraft. All testing was to be done with production aircraft rather than with "hand-made" vehicles. ("Hand-made" vehicles have played an important role in resolving early development uncertainties in many aircraft programs, for example the F-104, which we consider next.) Had all the initial predictions turned out to be accurate there seems little question that the program would have been cheap and quick as compared with programs involving a similar technical jump. Unfortunately, crucial predictions about the airframe, engine, fire-control system, and air-to-air missile turned out to be substantially wrong. So closely integrated was the planned system that each wrong prediction implied a delay, not only in the satisfactory completion of the component in question, but also in the development of other components. The telescoping of testing and tooling, finally, did not achieve the saving in time that it was intended to achieve; the unexpected need for the weight-reduction program meant scrapping the early tools.

The F-102 case strikingly illustrates one of the major external events that can make major revisions desirable. This event is the discovery, outside the project, of new principles that advance the whole state of the relevant art. Such a discovery was the "ideal-body" rule.

THE F-104

The F-104 series is related to the design Lockheed entered in the competition of 1950-1951 out of which grew the Convair F-102/106. After the screening in mid-1951, only Convair and Republic were left in the running. This came as a surprise and was a serious loss to Lockheed since at one point in the evaluation the firm had received a letter from the Air Force stating that Lockheed's proposal, the L-205, would be placed under development. With this turn of events Lockheed had fears for its long-run future in the fighter field. Its last fighter development prior to the L-205 proposal was the XF-90, which had been a failure partly because it was designed around the Westinghouse J-46 engine which, as we saw in Section III, did not perform as expected. Because of the failure of the XF-90, and the lack of a contract enabling Lockheed to keep its hand in the fighter field, Lockheed foresaw its future ability to compete with other companies for fighter business endangered.

In order to remain in the fighter field, Lockheed continued work on the L-205 and renewed its efforts to win a contract. A subsequent version growing out of this work, the L-224, won support in some circles of the Air Force in early 1952, but the model was judged by WADC to be not enough better than the Sabre 45 (the F-100) to justify its development.

Lockheed returned to the drawing board and in May 1952 presented WADC with another proposal, design L-227. This proposal, at one time on the verge of acceptance, was rejected in June. The rejection marked the emergence of a new concept that was to affect the type of aircraft Lockheed subsequently developed. In mid-1952, military circles were concerned about the possibility of future wars resembling the Korean War. Attention was focused, therefore, on exploring the implications of peripheral wars for the kinds of military hardware required. For a tactical fighter it was suggested that a cheap, mass-produced lightweight plane be developed. Proponents of this approach argued that it was profitable to trade quality for quantity,

and that we had gone too far in insisting that our planes be equipped with a number of "luxury items." This position was supported by the testimony of many ex-Korean combat pilots, who asserted that much of the equipment on their planes was of little value. This concept stood in direct contrast to all Lockheed's earlier proposals, which had called for gross weights in the 26,000-pound class.

The new proposal was far from unopposed. Another faction in the Air Force, contending that such a development policy would lead to inferior equipment and a second-best Air Force, strongly urged development of aircraft such as the L-227. The lightweight fighter proponents were strong enough at the time to block the contract with Lockheed for the L-227 but not strong enough to initiate development of a lightweight fighter. As a result, neither a heavy- nor a lightweight fighter contract was let in mid-1952.

The rejection of the L-227 by the Air Force touched off a lengthy debate over the relative merits of light- and heavyweight fighters. Within the Air Force the strongest supporters of lightweight fighters were those in the Tactical Air Command responsible for maintaining theater air superiority. Most strongly opposed were those in Air Defense, who insisted that they wanted an all-weather fighter, which in turn would require a heavyweight design. Within Lockheed, the emergence of the lightweight concept was viewed as a threat to its heavyweight proposals. Consequently, Lockheed stepped actively into the debate on behalf of the heavyweight design. Other companies, Northrop, North American, and Republic in particular, submitted specific lightweight proposals to the Air Force.

During the course of the debate certain developments in technology affected the relative merits of the two positions. The most important were in the field of engines, where it appeared that better thrust-to-weight ratios, lower specific fuel consumption, and better ratios of thrust to frontal area would shortly be available. These factors implied that a reduction in aircraft weight was possible with no reduction in over-all capability. Some of these advances had

already been embodied in the British Sapphire engine, the predecessor of the Wright J-65. Although the various models of the J-65 weighed about the same as most models of the J-35 and J-47 (the engine in the B-47), the J-65 provided a considerable increase in thrust. Specifically, the J-65 had 7800 pounds static thrust dry and 11,000 pounds thrust with afterburner, as compared respectively with 5400 and 7500 for the model 17 of the J-47. The specific fuel consumption of the J-65 was .93 dry at sea level and 2.0 with afterburner, compared respectively with 1.12 and 2.3 for the J-47. Furthermore, it appeared that even more substantial gains would be achieved with the General Electric J-79 engine, which was at that time in the paper stage. Developments in radar and armament also made weight reductions feasible without loss of capability. In particular, development of rapid-fire cannon made it possible to reduce gun weight without reducing fire power.

For these reasons, the lightweight proponents successfully argued that it was possible to build a first-class fighter much lighter than the F-86F and the F-100. Early in 1953, then, the Air Force was ready to let a contract for such an aircraft.

Although Lockheed had been a very active proponent of the losing side of the debate, it was quick to adjust to the turn of events. Lockheed took only about three weeks to come up with a proposal. The L-246, a 15,000-pound airplane, was hardly more than one-half the weight of the previous proposal. Contract negotiations between Lockheed and the Air Force proceeded in early 1953, and a letter contract was approved by the Air Force in March of that year. It covered the procurement of two prototype XF-104s (the redesignation of the L-246), mock-ups, spare parts to support 100 hours of flying, and rocket and wind-tunnel models, all for an estimated price of a few million dollars. The plane was to be equipped with a J-65 engine having a maximum thrust with afterburner of 12,000 pounds. Armament was specified. Empty weight and maximum take-off weight were to be 10,720 and 18,570 pounds respectively. For the basic mission (take-off weight

16,145 pounds), maximum speed was given as 1.82 Mach (1048 knots at 35,000 feet), sea-level rate of climb as 49,200 feet per minute, a combat ceiling of 52,900 feet, and a combat radius of 375 nautical miles.

The definitive contract, signed in November 1953, covered the same items but at a price several million dollars higher. The two prototypes were to be delivered in March 1955. A subsequent change in orders increased the cost coverage to include items such as modification of afterburners, and development of the fire-control system.

Construction of the two prototypes began in March 1953 and first flight took place only 11 months later, a month ahead of schedule. Total cost of the two planes, including development of the fire-control system and some flight testing, amounted to less than fifteen million dollars.

Lockheed's performance in constructing the XF-104 reflected a number of circumstances. In the first place Lockheed felt that its F-104 would be crucial in determining whether or not it stayed in the fighter business. Consequently, it took the project very seriously. In the second place, whether by accident or intention, Lockheed was given a relatively free hand in development. The requirement for the system, issued almost simultaneously with the contract, imposed few constraints on Lockheed; it had little to say in detail, being couched for the most part in generalities. In the third place there was extensive and very fruitful use of wind tunnels. Lockheed was able to rely upon testing facilities of the National Advisory Council for Aeronautics to resolve uncertainties associated with the F-104. Fortunately for Lockheed, NACA was particularly interested in a number of problems that were of concern to Lockheed in developing the F-104. By comparison with most aircraft under development, the F-104 was therefore able to command an unusual amount of wind-tunnel time for proving its aerodynamics. In the fourth place Lockheed's task was made substantially easier by the availability of information, particularly wind-tunnel data, gathered in the course of the Douglas X-3

program. Although the X-3 program (initiated in 1943 and terminated in 1951) was generally thought a failure, largely because it was designed around the ill-fated J-46 engine, the experience gained had a carry-over value for the F-104 program. Finally, the vehicles were virtually handmade, few tools being designed specifically for the purpose of constructing it. Later when Lockheed did in fact tool for production, it took as long as a year and a half to construct some of the tools required. Had the first two test aircraft awaited production tooling, they would have been available only much later than they were.

In addition to building an aircraft in less than a year, a record unequalled by any other of the "Century Series" aircraft, Lockheed also developed an afterburner for the engine. When the J-65 was delivered in the fall of 1953 for use in the XF-104, it was a relatively proven engine, having passed its 150-hour test in its non-afterburning version. An afterburning version was being readied by Wright for the Navy, but not on a schedule suitable for use in the XF-104 prototypes. Because the success of the F-104 program was at stake, Lockheed itself undertook the development of the afterburner and succeeded in making available a J-65 afterburner version for the early XF-104 flight-test program.

The XF-104 made its first flight in February 1954 and completed Phase II testing 13 months later. Most deficiencies in the aircraft's performance, shown in Table 7, were attributed to the low power of the J-65 engine, even in the afterburning version. The airframe itself was highly praised.

In mid-1954 the Air Force was undecided about the program. The old heavyweight proponents were in favor of dropping the project. A second group, concerned with the possibility that the United States might soon become involved in a war in Indochina, contended that the F-104 should be procured immediately with the J-65. A third group, whose views eventually prevailed, suggested adding more equipment to the F-104 and improving its performance by switching to a more powerful engine. These suggestions were supported by the fear that with

Table 7

PERFORMANCE OF THE F-104

	Actual (Phase II flight test)	Specification 24 March 1953
Engine thrust (pounds)		
Maximum	10,300	12,000
Military	7,800	8,300
Basic weight (pounds)	11,800	11,406
Sea level rate of climb (feet per minute)	32,000	49,200
Combat ceiling (feet)	48,650	52,900
Maximum speed (Mach)	1.59	1.82

the J-65 engine the F-104 would have inadequate altitude capability in air defense. Enemy bombers, it was argued, would very likely attack from an altitude in excess of 50,000 feet and would, therefore, be invulnerable to attack by the F-104.

In view of the relatively low thrust of the J-65 engine, an official decision was made in mid-1954 to switch to the J-79 in the production version. At the estimated 14,350 pound maximum thrust, the airplane was expected to attain Mach 2, have a rate of climb of 20,000 feet per minute at 35,000 feet, and have a combat altitude of 60,000 feet.

There was considerable risk associated with substituting the J-79 for the J-65. In mid-1954 a 50-hour test had not been completed on the J-79 while the J-65 had already passed its 150-hour test. Yet it was felt that the expected increase in performance was sufficiently great to warrant taking the risk.

An implementation program involving obligations for production planning and tooling was begun in mid-1954. In the fall of 1954 Lockheed signed a \$39 million contract for 17 F-104A airframes. Deliveries were run from January to October 1956.

As a result of the switch to the J-79, the F-104 fuselage was lengthened by five feet and increased in diameter. From the point of view of production, the J-79 version was virtually a new airplane. If Lockheed had tooled for production of the J-65 version, a switch to the J-79 would have meant scrapping nearly all these tools.

The first F-104A made its first flight in February 1956, two years after the first XF-104 flight. The early flight-test program of the F-104A suffered considerable delay, at least partly because of some difficulties with the J-79 engine. Development of the engine was not yet complete; the main engine difficulties were resolved when the J-79 passed its 150-hour test in June 1956. The F-104 attained a progressively higher top speed in subsequent tests, attaining 1.95 Mach late in April. By the first week in June, just prior to the

delivery of the second F-104A, the plane had made 16 flights. Phase II flight testing was completed six months after first flight.

The test evaluation disclosed deficiencies in the new airframe somewhat different from those found by the test on the XF-104 model. While the two principal deficiencies of the F-104A were lack of range due to low fuel capacity and "pitch-up" (an aerodynamic problem), neither was mentioned in the earlier evaluation. The latter was largely caused by the combination of the higher thrust engine and the peculiarities of the modified airframe, which gave the aircraft some of the characteristics of a ballistic missile. When pitch-up occurred, the wing blanketed the air flow to the empennage, the pilot lost virtually all control of the aircraft, and because of its extreme speed and inertia the aircraft followed a ballistic flight trajectory.

However, a good deal was learned from the XF-104 experience that was applicable to the F-104A program. Although much of the detail engineering planning that went into the XF-104 -- the drafting and blueprinting -- was not transferable in a body to the F-104A program, Lockheed did learn from the XF-104 a considerable amount about general manufacturing problems, about how to handle the metals going into the aircraft, and about particular tooling problems. For example, the fuselage of the aircraft is built around a central keel that is the main structural weight-bearing member. Since this feature appeared in the XF-104, Lockheed gained early experience with it and found the experience applicable to the F-104A program. Furthermore, test flying the XF-104 disclosed certain deficiencies that could be corrected; hence it was possible to some extent to accelerate the test-flight program for the F-104A. This carry-over from the XF-104 may partly explain the seven month difference between the time taken to carry the XF-104 through Phase II flight testing and that taken by the F-104A.

Early wind-tunnel tests had shown that there might be a pitch-up problem, although it did not appear in the earlier Phase II evaluation. It was not until early 1957 that development of a satisfactory pitch control was completed for the F-104A.

Unexpected major difficulties with the designated gun also complicated the F-104A program. The aircraft reacted in an undesirable way to firing. These difficulties were partially responsible for the decision to replace the gun in Air Defense Command (ADC) interceptors with two Sidewinder infrared missiles. While the F-104 was originally designed to serve the Tactical Air Command, ADC later decided to procure four squadrons, but for various reasons did not favor use of the gun. Besides the mechanical difficulties with the gun, ADC felt that guided-missile armament was superior in air defense and that provision for guns on the F-104A would require a return to the ground-support provisions that ADC had long since abandoned in its general change-over to missile and rocket armament for all other interceptors.

Although subsequent changes in force requirements sharply reduced the value of the F-104 to the Air Force, the generally high capability of the F-104 system as a lightweight, low-cost fighter accounts in part for Lockheed's recent success in the stiff competition among U.S., English, French, and Scandinavian manufacturers to sell lightweight, low-cost fighters to NATO and SEATO governments. The West German Air Force procured a license from Lockheed for manufacture of the aircraft in Germany. A number of other countries have also procured the aircraft.

CONCLUSIONS

The F-104 history illustrates that research and development in one program can have a great carry-over value to another. Lockheed's success in building and flying a prototype less than a year after go-ahead would very probably not have been possible without the knowledge derived from the Douglas X-3 program. Although the value of this experimental effort in the F-104 effort could hardly have been anticipated when Air Force money was advanced to finance the program, nevertheless the value to the Air Force of the X-3 program extended far beyond the immediate results achieved with it.

The F-104 history also illustrates the inevitable uncertainties of development. The unforeseen difficulty with the chosen gun is

typical of the troubles that can beset a program when reliance is placed on components that are only in early development at the time the aircraft program is initiated. Although the aircraft was first envisaged as an air-superiority fighter, it has gone into ADC inventory as an interceptor, and in the tactical version it was given a nuclear bomb capability. Its major customers turned out, in the end, to be foreign governments. Although it was originally designed to have only gun armament, the Sidewinder missile (which was developed quite independently) has been successfully added. Although the aircraft was originally designed for considerably less than Mach 2 performance, the engine switch provided maximum speeds and rates of climb and ceilings (zoom climb) that broke speed and altitude records for operational USAF aircraft.

A notable aspect of the F-104 program is the manner in which it was conducted. The initial commitment to Lockheed covered construction of two experimental prototypes that were virtually handmade. This had two advantages: first, Lockheed was able to get the aircraft flying more quickly than would have been possible if production tooling had been used. Furthermore, the absence of tooling greatly facilitated the switch from the J-65 to the J-79. The merits of lightweight fighter design were quickly and (relatively) inexpensively tested in an early flight program, and modifications were added to the production version at modest expense. Although the initial financial commitment to Lockheed was small (with the emphasis placed on early experimental flight test rather than on production of an operational weapon system), the total time of five years between the start of the program in 1953 and the first squadron deliveries in 1958 is a record exceeded in the Century Series by only the F-100A.

V. TWO BOMBER PROGRAMS

(Based in Part on Studies by W. Meckling)*

We turn now to two other aircraft programs, those in which the first two postwar strategic jet bombers were developed.

THE B-52

Early Development History

The B-52 was developed under military characteristics issued November 23, 1945. Those characteristics specified a plane with the following as "minimum" performance requirements:

- | | |
|--|------------|
| (a) High speed at tactical operating altitude for 15 minutes | 450 mph |
| (b) Tactical operating altitude | 35,000 ft. |
| (c) Service ceiling | 40,000 ft. |
| (d) Service ceiling, one-half of engines | 15,000 ft. |
| (e) Tactical operating radius with 10,000-pound bomb | 5,000 mi. |
| (f) Average speed for above radius | 300 mph |
| (g) Take-off over 50-foot obstacle at design gross weight | 7,500 ft. |
| (h) Landing over 50-foot obstacle at design gross weight less droppable fuel and bombs | 4,500 ft. |

In addition, the characteristics set forth some specific and some general requirements regarding armament, crew, equipment, and structure and design features.

A directive inviting design proposals was circulated February 13, 1946. The letter accompanying the directive stated:

It is desired that the requirements set forth be considered as a goal and that the proposal be for an interim airplane to approximate all requirements, except that emphasis must be placed on meeting the high speed requirement ...It is the intent that design proposals should

*With supplementary sections based on histories prepared by Wright Air Development Center.

present the best possible over-all airplane consistent with power plants or combinations of power plants that will integrate with combined Phase I and Phase II program of approximately three years.

Boeing, Convair, and Martin responded with cost quotations and preliminary designs. The evaluation that followed concluded in favor of Boeing's model 462, and a Phase I development contract was authorized. The price quoted by Boeing in its proposal dated April 18, 1946, for the Phase I work, was \$1,785,176. It was notified of its award on June 5, 1946, and a letter contract was approved June 28, 1946.

The most prominent feature of the early history of the B-52 is the succession of major design changes that occurred. From the time Boeing was adjudged the winner in the original design competition until the swept wing turbojet version finally prevailed, one configuration after another was proposed, studied, and then supplanted by the next. Some of these never went beyond paper studies, others were the subject of Phase II development contracts. Table 8 summarizes pertinent physical and performance characteristics for some of the models.

The ink was hardly dry on the contract authorizing Boeing to go ahead on model 462 before dissatisfaction with the B-52 became apparent. The office of the Assistant Chief of Air Staff, Major General E. E. Partridge, indicated in September 1946 that the B-52 was "an unrealistic type" because of its monstrous size, and that the B-52 design failed to meet Army Air Force requirements. At a Wright Field conference held on the 17th and 18th of October in 1946, Boeing proposed model 464, and on November 26th, General Craigie, Chief of the Engineering Division, recommended that the B-52 development be converted to essentially that design.

However, at another conference held the next day the whole concept of the B-52 was changed materially. Instead of developing a general purpose plane, it was decided to make the B-52 a special purpose plane designed for the specific mission of carrying an atomic bomb over a long range, taking advantage of surprise, apparently by night. The minimum requirements for crew and armament were to be

Table 8
B-52 MODELS -- DESIGN DATA

Model Number	Date	Engine Type	Number of Engines	Design Gross Weight	Radius (statute miles)	Maximum Speed	Cruise Speed
462	April 1946	T-35-1	6	360,000 (alt.) 400,000	3,110 3,570	440	410
464	November 1946	T-35-2	4	230,000	2,500	430	400
464-17	February 1947	T-35-3	4	400,000 (alt.) 480,000	3,750 4,610	440	420
464-29	? 1947	T-35-3	4	400,000	5,005	455	415
464-35	January 1948	T-35-3	4	280,000	3,540	512	500
464-40	July 1948	J-40-WE-6	8	280,000	3,375	536	500
464-49	October 1948	J-57-1	8	330,000	3,060	572	520

reduced substantially, thus permitting a large weight reduction. The airdrome problem was also to be alleviated by equipping only a few wings.

In an effort to meet this new concept, Boeing proposed two models, 464-16 and 464-17, which were essentially alike, except that 464-17 was still a general purpose plane with a maximum bomb load of 90,000 pounds; whereas 464-16 could carry a maximum of only 10,000 pounds. At a conference on January 7, 1947, it was decided that Boeing should go ahead with model 464-17, the general purpose version. New military characteristics superseding those of November 1945 were issued June 23, 1947, reflecting the new version of the B-52. Working toward the desired 10,000-mile range, Boeing evolved model 464-29, which was to use a more advanced type of T-35 engine. The 464-29 was the first model that purported to meet the range requirement.

The new B-52 configurations by no means assuaged all fears regarding the advisability of its development. Heavy pressures for a change became evident again in the summer of 1947. In September 1947 a Special Bombardment Subcommittee of the Aircraft and Weapons Board drew up and recommended a new set of military characteristics that reduced the required range from 10,000 to 8,000 miles; increased the required cruising speed to 550 mph, and specified air-to-air refueling. Subsequently the cruise speed requirement was reduced to 500 mph, with a maximum speed of 500 + mph. New military characteristics embodying these changes were issued December 7, 1947.

Some controversy and confusion over whether a new competition should be held followed the issuance of the new requirements. At one point the B-52 contract was ordered canceled; but the cancellation was later rescinded and Boeing was authorized to proceed with a new model. The new model was 464-35, a four-engine, straight-wing turbo-prop still utilizing the T-35-3 engine.

In May 1948, Boeing discussed with Wright Field personnel a preliminary study of the B-52 using J-40 turbojets, and was requested at

that time to expand the study. The results, which appeared encouraging, were sent to Wright Field in July.

When Boeing personnel came to Wright Field on Friday, October 22, 1948, to discuss the turboprop model 464-35, they were informed that the Air Force was seriously interested in a turbojet. The Boeing officials drew up the design and constructed scale models of the present B-52 with swept wings and eight J-57 engines. In January 1949 the Board of General Officers met and decided to let Boeing go ahead with model 464-49 in place of the turboprop without a new design competition.

The B-52 configuration was not yet settled. Dissatisfaction still existed, especially with regard to radius, which at that time was estimated at 3,050 miles. In November 1949, Air Force Headquarters expressed dissatisfaction with in-flight refueling as a means of getting the required range, but agreed to accept a B-52 if it had a radius of 4,250 miles. Boeing engineers responded by increasing the take-off weight to 390,000 pounds, thereby increasing the radius not to the figure requested, but to 3,530 miles. Only in a much later version, the B-52C (weighing 450,000 pounds), did the radius reach an estimated 4,170 miles.

Development History After 1949

Even with the essential configuration decided upon -- the swept-wing design, the engines, the intended weight, radius, and speed -- numerous uncertainties remained. Although the flights of the B-47 (starting in 1947) had resolved some major uncertainties about large, swept-wing aircraft, numerous problems of fabrication and detailed design peculiar to the B-52 had to be solved. Fairly confident predictions about the J-57 engine could be made (at least by the developing company) for early versions of the engine had been running for some time. In the face of the remaining uncertainties the Air Force was committed, at the end of 1949, to two prototype aircraft, the XB-52 and YB-52. It appeared to be the intention, at least in 1950, that

a production commitment was to await the flying of at least one of the prototypes.

Early in 1950 it was decided that both prototypes should use not the nearly qualified first model of the J 7, but rather the more advanced J-57-3 which the production planes were later to use. In July 1950, an engine mock-up inspection board, observing the engine mock-up as well as the relevant information acquired in developing the preceding J-57-1 version, concluded that the range requirement could be met with the engine. Shortly afterward several problems arose in developing the J-57-3 model, notably a surging condition during acceleration and deceleration. These fairly normal engine difficulties might themselves not have caused delays in the test flights of the two B-52 prototypes had not two other changes occurred: (1) The J-57 became increasingly important to other aircraft programs (the F-100, for example); and (2) the entire B-52 program was accelerated, in part, presumably, because of the pressures generated by the Korean War. While the first test flight had been scheduled for February 1952, Boeing announced, in early 1951, that it hoped to make a test flight before the end of the year.

The tightness of engine deliveries, however, caused this prospect to vanish. It was in April 1952 that a prototype (the YB-52) first flew. Engines had to be used that did not yet contain the large bleed valves chosen to solve the surge problem. Speed and altitude were consequently restricted. The engine scarcity problem continued until early 1953 and somewhat hampered the YB-52 flight test program.

The role of the swept-wing B-47 in resolving aerodynamic uncertainties throughout the period 1949-1952 was an important one. By the time Boeing was ready for the first B-52 flight tests, the B-47 had had more than four years of flight tests, and many difficulties of large swept-wing aircraft had come to light. A number of important "fixes" were copied from the B-47 experience to solve difficulties that appeared in the B-52 flight tests.

The YB-52 flight test program (despite the late availability of suitable engines) was considered to be unusually rapid. Its results were encouraging, although a number of needed alterations were discovered. The other prototype, the XB-52, though built earlier than the YB-52, first flew in October 1952. The problem of engine surge did not disappear for a few more months and the XB-52 test program was hampered, as the earlier test program had been. Engine company facilities were still strained by competing demands and it therefore took fairly long to find an appropriate fix. The initial solution (large bleed valves) turned out not to work well. Other serious problems arose in connection with crew safety (leakage of fumes into the cabin) and flight control surfaces.

By May 1954, however, most flaws had been corrected. Air Force opinion about the plane had become enthusiastic and one Air Force general in the Air Materiel Command is reported to have suggested that "someone should try to discover how we accidentally developed an airplane that flies so beautifully."

What production commitments were made through the years 1950-1954? The acceleration that occurred in 1951 meant the abandonment of the plan to let flying prototypes precede production commitments. In February 1951 the Air Force and Boeing signed a letter contract for 20 production aircraft. In May 1951 there was a mock-up inspection, and in October 1952, after the first YB-52 flights, final production specifications were approved. They differed somewhat from those of the letter contract but not enough, apparently, to require any scrappage of tooling already accumulated. The first production airplane rolled from the plant in May 1954, and the first delivery of a B-52 to the Strategic Air Command took place in June of 1955. This was ten years to the month after the preliminary conference out of which grew the first military characteristics for the B-52. It is most unlikely that anyone at the earlier date had in mind a plane very much resembling the one delivered.

Remarks on the Early History

It is clear that the basic design of the B-52 changed frequently in the years 1946-1949, and that differences between basic designs were not negligible. The Air Force did not start out by analyzing the alternatives, selecting the one "best" alternative, laying down a detailed schedule of development and then proceeding forthwith to build a number of prototypes, for the Air Force could not be certain what was best; and the best guess kept changing as new knowledge was acquired.

In retrospect it is difficult to appreciate the magnitude of the uncertainty about what kind of bomber should be developed. In early versions, for example, the central question was whether the Air Force was buying a useful capability by insisting on a plane with a range of 10,000 + miles. There was disagreement as to how much such a plane would have to weigh, and hence disagreement about how much it would cost. The cost disagreement included not only the airframe, but also cost of facilities, bases in particular. Those who felt the plane would be heavy and expensive argued that the Air Force would do better to spend money developing a lighter, shorter range bomber. Similarly there was uncertainty about how much speed should be sacrificed in order to achieve range. Some argued that a 10,000-mile bomber would be so slow that its vulnerability would render it ineffective. In regard to configuration there was the question of the flying or delta wing versus the swept or straight wing. The engines furnished another major source of uncertainty: should they be turboprop or turbojet? On all these questions and many more, individuals or organizations voiced concern, and sometimes vigorous controversy. Because of these uncertainties the Air Force accepted any particular concept only tentatively. It did not treat development as though the central problem was simply to get "a B-52" as quickly as possible. Rather it concerned itself with the question of discovering what B-52 would in fact be best.

Moreover, as efforts to develop a plane proceeded, new knowledge was constantly being acquired that impinged on the current program.

Although the period from 1945 to 1949 was hardly one of unusually rapid technological change (there was nothing that might be described as a technological breakthrough) in just a few years the technological outlook for bombers changed materially. Gradually the Air Force was better able to estimate weights, costs, timing, the penalties imposed by large crews, and the penalties imposed by all-around defensive armor. The success of swept wings became apparent with tests of the B-47. Enemy defensive capabilities were reevaluated. Development work at Pratt and Whitney disclosed the possibility of obtaining very significant advances in the performance of the turbojet.

Thus the central theme in the early B-52 development is the process of revision based on new information. The proposal of the much lighter, shorter range model 464 reflected a conviction that the long-range version was not optimal. This conviction had been materially influenced by a growing feeling that a 10,000 + mile bomber would be much heavier and more costly than had been anticipated. The original stimulus for model 464-17 was similar. In this case the answer to excessive costs and weights was to sacrifice defensive armament, bomb load, crew comfort, and crew size in order to preserve range. As it turned out, however, new engines appeared on paper at least, and a new general purpose plane resulted. Model 464-29 represented no new change in concept, but did reflect improved aerodynamics, and once again a new engine.

New knowledge of two classes was important in the adoption of model 464-35. For one thing, great strides had been made or were within sight in air defense, and it was clear that the slow speed of the long-range plane was going to be a serious handicap. For another, aerial refueling developed and began to appear much more attractive as a means of achieving long range than before. Model 464-40 occupies a unique position in this list, because it was never seriously considered as a plane to be developed. Its purpose was purely to investigate the feasibility of turbojets. Model 464-49 grew partly out of the studies of 464-40, but was influenced heavily by early paper studies

of the J-57 engine, and by the success of the swept-wing B-47 which had first flown in December of 1947. Furthermore, it had become quite apparent by this time that developing a 500 mph turboprop engine, gear box, and propeller was much more difficult than had been anticipated.

In retrospect it might seem that the factors that resulted in the decision on the final model of the B-52 should have been apparent much earlier. But the uncertainties that plagued the decisionmakers were not of a kind that might be quickly and easily resolved. Even the best experts could not agree, and as a result the advice the Air Force obtained on technical matters was equivocal. On the question of weight, for example, studies by Boeing yielded weight-range curves considerably more optimistic than similar studies made elsewhere, and the question of which was more nearly correct was a very controversial matter. On the question of configuration, there were many experts backing the flying wing or delta wing for long-range bombers. And it was not until December 1947, when the B-47 first flew, that flight data began to be available on the performance of the swept wing for large aircraft. And then, of course, there was the question of which type of engine: turboprop or turbojet? In terms of potential advantages, it could be quite convincingly demonstrated, the turboprop was a better choice. But despite the fact that in the period 1946 through 1948 about 60 per cent of the engine budget was being used for turboprop development, it was far from clear when a satisfactory turboprop would be available. On the other hand, it also was very uncertain if within a reasonable period of time a turbojet could be developed to power a bomber over the "minimum" range believed necessary. As late as 1948 a respectable body of opinion held that such turbojets were very far in the future. It is clear that the picture painted for turbojet development relative to turboprop was much blacker than what actually proved to be the case.

Inability to foresee the future was not limited to technical matters, however. It is doubtful if anyone in those early years could have foreseen very accurately the political and strategic environment

in which the B-52 was destined to operate. Many of those who became the strongest proponents of a large B-52 force then contemplated a small one.

Remarks on the Later History

After the decisions of 1949 the Air Force, it seems, was well aware that uncertainties remained. It was decided to try to continue developing the B-52 so that changes could be made relatively easily. The backlog of B-47 experience, moreover, decreased the likelihood of major design changes. The originally unintended acceleration of 1951 contributed heavily to at least one of the major development difficulties (the scarcity of suitable engines). Even so, however, major difficulties following the flying of prototypes were few compared with other bomber programs -- the B-47, for example.

THE B-47

The B-47 grew out of military characteristics issued in November 1944. They called for a plane having a minimum high speed of 500 mph, a tactical operating altitude of 35,000 to 40,000 feet, and a range of 2,500 to 3,500 statute miles. The first version proposed by Boeing in December 1944, and accepted by the Air Force, was model 432; a straight wing, four-engine turbojet, with the engines mounted on top of the fuselage. However, in 1945 on a visit to Germany, Boeing's chief engineer uncovered valuable information on the performance of swept wings. This prompted Boeing to propose model 448, which had swept wings and two additional engines in the tail. Finally, in the fall of 1945 model 450 was proposed and accepted. It was a six-engine version with the engines slung on wing pylons essentially like the B-47 that finally emerged.

The swept-wing design promised to permit attainment of high speeds, given the performance expected of advanced versions of the existing J-35 engine. There were, however, major aerodynamic uncertainties in applying the swept-wing principle to aircraft of this size as well as some engine uncertainties. Accordingly, the early stages of the

development of the B-47 were conducted on an experimental basis. There were only two prototypes, the XB-47s, and these were essentially stripped down experimental planes. This meant that the cost of buying the early flight test information was very small. The total amount paid to Boeing for the two prototypes was in the neighborhood of \$12 million, but there was a substantial amount of government furnished equipment. According to B-47 project personnel the cost of that equipment was probably equal to the amount paid Boeing. This would put the total cost somewhere between \$20 and \$30 million, a very low figure compared to later programs.

The first XB-47 flew in December 1947. Subsequent test flights in the next few months established that the aircraft would perform satisfactorily in the 600 miles per hour speed range. Fourteen months after the initial flight the XB-47 established a transcontinental speed record. The plane also "ran away" from the currently operational jet fighters.

A number of changes were still needed to make the aircraft suitable for mass production and operational use. For example, a rocket-assisted take-off device, not yet developed, appeared necessary in order to get the fully loaded combat version off the ground. Nevertheless, basic airframe and engine uncertainties had been resolved when the first production contract was signed. This occurred in November 1948 and covered the procurement of 10 B-47As and later procurement of 41 B-47Bs, with the delivery period extending from January 1950 to March 1951; the initial delivery date was soon changed to April 1950. The B-47B was to have more powerful engines than the B-47A or the XB-47; but the intended engine, designated the J-47, had the same basic design as the J-35 that powered the X and B versions. The B-47B was also to have more equipment, notably in-flight refueling equipment, than the B-47A; the B-47A, in turn, was to have more equipment than the XB-47. Otherwise the three versions were to be very similar. Most of the B-47As and a number of the B-47Bs were intended not for immediate operational use but for flight-test programs, designed to eliminate inevitable "bugs."

Compared with its subsequent revised form the first production schedule was a rather relaxed one. It seemed reasonable to expect that the remaining engineering problems could be solved without much delay beyond the intended delivery schedule.

By June 1949, however, the number of B-47Bs ordered had approximately doubled, with the delivery period extended only to December 1951.* This was the first acceleration of the original production schedule. The outbreak of the Korean War in June 1950 led to much more acceleration. In December 1950 the Air Force began to seek B-47 production sources in addition to Boeing. Douglas and Lockheed were selected. By April 1951 the number of B-47Bs on order (including a reconnaissance version) had risen to 1500. A high monthly production rate (utilizing a six-day week) was authorized for all the producing plants.

At the same time there was heavy emphasis on getting the aircraft from factory to combat readiness as soon as possible. A number of aircraft previously designated to serve initially for testing were now assigned to operational use. Installations of several of the later models of the J-47 engine were made in planes destined for operational use before these models had passed their 150-hour tests. There was a similar acceleration with respect to the rocket-assisted take-off device and other components.

As a result the early operational history of the B-47 was marked by a series of delays, equipment failures, equipment deficiencies, substitutions of provisional equipment, equipment voids, and so forth.

* At about the same time, Boeing became interested in the promised J-40, at that time a paper engine, whose ultimate fate was described in Section III above. If it met specifications, the high-thrust version of this engine would achieve better fuel consumption and range than the J-47. This possibility did not lead to a change in the intended engine for all the production aircraft on order, but only (in August 1949) to the decision that one B-47B should be fitted with J-40s for test purposes. In late 1950 the dubious future of the J-40 led to cancellation of this plan.

Early production aircraft had no ejection seats; an interim fire control system substituted for a development that failed completely; the bombing system was so unreliable as almost to destroy the usefulness of the plane as a bomber; there were dangerous fuel tank leaks; canopies cracked and blew off; and there were numerous other major and minor difficulties.

At the same time that the decision was made to seek production sources in addition to Boeing, plans were begun for a modification program. It was realized that those B-47s delivered late in 1951 and in 1952 would need modification in order to incorporate the major engineering changes that would be made during this time. A retrofit center was established in late 1951 and two more centers in 1952. Several hundred aircraft passed through these centers at costs ranging between one-tenth and one-fourth of the estimated cost of the unmodified aircraft.

The delays, difficulties, extra costs, and general dissatisfaction to which the accelerated production program gave rise should not be allowed to obscure the advance achieved by the B-47 nor the flexible manner in which the program began and might well have continued. The United States was much earlier than either Britain or Russia in having a medium jet bomber operational. It is safe to say that since World War II no other advance (except perhaps thermonuclear weapons) has given the United States a comparable strategic advantage.

Until the attainment of a flying prototype, aerodynamic uncertainties were large because of the novelty of the basic design. Requirements, on the other hand, were broad and commitments small; the use of new information was possible without major costs of revision. It was the deliberate speeding up of production that broke this pattern.

VI. THE SIDEWINDER MISSILE

(Based on a Study by T. Marschak)

Finally we consider an air-to-air missile program that seems to have been a classic example of an important type of strategy, and had striking organizational properties as well.

DEVELOPMENT HISTORY

Sidewinder, a passive, infrared-homing, air-to-air guided missile was developed at the Naval Ordnance Test Station (NOTS), China Lake, California. The outstanding characteristics of the development program were as follows:

(1) Only very broad requirements were imposed on the missile throughout its development. The goal was a simple, reliable, inexpensive, and easily maintained missile for use primarily in tail-chase attack from single-place fighter aircraft. In this use it was to have better kill probabilities than existing weapons for some useful altitude and speed ranges. More detailed performance specifications were not imposed and neither were dimensions or weight. No specific using aircraft (with accompanying specific stowage requirement) was designated.

(2) The developing laboratory had authority for all design decisions and was free to pursue parallel approaches to the design of individual components, using contractors if desired.

(3) Emphasis was placed on early testing of all components and early and numerous firings of test missiles. Extensive test facilities were available at the developing laboratory.

Origins

The history of Sidewinder begins in 1947 with a NOTS survey of air-to-air homing devices. The survey concluded that infrared homing held great promise for accurate and inexpensive missiles. According to another, roughly contemporary NOTS study, a pursuit tail-on attack (hence an attack aimed at the target plane's principal infrared source) was the optimum interception path for a fighter against a high-speed

bomber. In 1948 a third NOTS analysis, concerning fire-control systems for air-to-air rockets was completed. The study found that by far the largest source of error in fire-control of rockets is movement of the target after firing; such errors far outweigh the errors due to computing, or to angle of attack, aircraft skid, and rocket dispersion. It was concluded that putting the guidance in the missile was a far more promising approach to reduction of error than that alternative of making fire-control systems vastly more complex (and hence unreliable).

In the same year, laboratory models of an infrared tracking device and of a hot-gas servo control valve (a promising means of steering a missile) were demonstrated at NOTS, and a target-radiation survey showed that radiation levels from jet aircraft were high enough so that existing photo-sensitive lead sulphide cells could be used as target detectors.

In June 1949 these studies and demonstrations resulted in a formal "Proposal for a Heat-homing Rocket." The proposal was written by W. B. McLean, who became the chief organizer of the Sidewinder program; it contained the basic design principles of Sidewinder.

Basic Design Principles

(1) Guidance is based on a gyro-stabilized target detector in which infrared radiation is converted into a usable signal as it passes through a rotating chopper and strikes a lead sulphide cell. The detector is not mechanically coupled to the missile body, so that tracking is independent of the body's motion. Oscillation problems which such coupling has generated in other missile programs are eliminated.

(2) A torque-balance control (servo) system governs the missile's steering fins (the fins assume an angle for which the control torques exerted on them balance the aerodynamic lift forces). Variations in required torque are very small compared with the variations in fin position required in a control system that directly commands fin angle rather than torque. Such large variations have posed

serious problems in other missiles. Moreover, the torque-balance principle eliminates the need for gain compensation (a further problem of other control systems) to allow for changes in velocity or altitude.

(3) The torque-balance servo system generates torques on the steering fins which are kept proportional to the torques generated by a second servo in the guidance unit. The second servo's function is to keep the seeker head always aimed at (precessed toward) the detected signal. The result is "proportional navigation" (the missile's flight path is turned at a rate proportional to the seeker's precession rate), so that the missile takes a collision-intercept course relative to the moving target. The alternatives of pursuit navigation and constant-bearing navigation are more difficult to achieve.

(4) There is a single primary energy source -- a gas-generating grain of chemical fuel -- for the entire guidance and control system. Difficulties of multiple power sources are avoided, and the use of a pneumatic control servo (the torque-balance servo) actuated by the generated gas eliminates problems of valves, pressure maintenance, pipes, oil cleanliness, and so on that have arisen in other missiles containing complex hydraulic servo systems.

(5) The guidance and control components are placed in the easily detachable forward section of the missile. Other detachable sections contain the warhead, an influence fuze, the rocket motor, and the rear stabilizing fins. The field-maintenance problems associated with electrical and hydraulic connections between sections (which have arisen elsewhere) are avoided.

(6) A previously developed and proven on-the-shelf rocket motor propels the missile.

(7) Forward concentration and detachability of the guidance and control section suggested use of a canard-type airframe. The warhead and rocket motor form part of the airframe and the rocket motor chiefly determines the missile's length. For this length the forward controls of the canard-type airframe, together with independent gyros placed in the rear fins, achieve aerodynamic stability.

Stabilizing tubes and gyro circuits, which are required in missiles with rear control surfaces, are avoided.

(8) The only missile control in the launching aircraft is a button that causes ignition of the propellant grain and so launches the missile. The button is pressed after visual sighting and verification that the missile is "locked on" to the target (a simple circuit delivers an audible signal to the pilot when "locking on" occurs). Once the button is pushed the aircraft neither controls the missile nor illuminates the target.

The overriding considerations in the choice of these basic design principles were simplicity and the avoidance of complex approaches that had caused difficulties in other programs.*

Initial Authorization and Funding

The work preceding the formal Heat-homing Rocket proposal had been carried on from NOTS Exploratory and Foundational Research Funds and had included voluntary after-hours work by a number of laboratory scientists. After submission of the proposal, Bureau of Ordnance officials felt that because of the number of untested principles involved, further work should continue with the same funding plus some additional Bureau support for fuzing development. Accordingly, the ensuing effort was aimed at demonstrating the feasibility of proposed but untested principles.

In the spring of 1950 an airborne detector detected a jet aircraft three-quarters of a mile away. Also in 1950 there was a demonstration of a laboratory hot-gas control servo that was sufficiently advanced for use in a practical missile. In the same year it was demonstrated that, given the missile length imposed by the choice of an on-the-shelf rocket motor, aerodynamic stability could be

* The Heat-homing Rocket proposal included also a free reference gyro and a fire-control system in the parent aircraft for initial correction of the infrared-guidance path. Both were dropped in 1950.

obtained by installing gyro wheels, powered directly by the airstream, in the rear stabilizing fins. Test missiles were ground-launched to demonstrate roll-control feasibility.

In March 1951, two tests of sun-seeking, free-flight, ground-launched missiles demonstrated that the hot-gas servo could control the missile in flight. Later in 1951 two complete, packaged seeker heads, each of different design, demonstrated tracking ability against light-bulb targets. One seeker (Type A) was designed at NOTS and had essentially the design given in the original Heat-homing Rocket proposal; the other seeker was developed by Avion Company, a contractor. The NOTS design exhibited a wobble, subsequently eliminated. Design studies of two additional seeker systems were initiated, one at Eastman Kodak and the other again at NOTS. The four competing designs differed with respect to independence of the optics from the gyro and with respect to the precession system (magnetic or pneumatic) used. (The design finally perfected and adopted, however, was the Type A design of the original proposal.)

In the autumn of 1951, as a result of the demonstrations, the NOTS group felt "technically certain that all problems were capable of solution if a full-scale program could be established" and proceeded to convey this conclusion to various groups within the defense establishment. They included the Bureau of Ordnance, the Guided Missile Committee of the Research and Development Board, and the presidentially appointed Guided Missile Coordinator. Money for new missile development was at that time very hard to obtain; support had to be sought at levels higher than the Bureau of Ordnance, and the novel contributions of the proposed program compared with others had to be stressed. The Guided Missile Committee approved the proposed program late in 1951, and Bureau of Ordnance funding of Sidewinder on a development level began with an initial budget of \$3,000,000.

The Bureau of Ordnance, however, still declined to issue a requirement imposing performance, size, and weight specifications. Requirements of that sort have constrained other missile programs starting at a far earlier stage of development.

Main Events in the Chronology of Full-scale Development

In 1952 a redesign of the airframe followed free-flight and wind-tunnel tests. The most promising seeker head (Type A) was improved and the turbogenerator, supplying all electric power in the missile and dependent on the same hot-gas source used by the control servo, was successfully adapted from a British design. In August 1952, the first air firing of a complete missile (using the Type B seeker, later rejected) took place, and in November the first of 30 more custom-made missiles (those with Type A seekers) was delivered.

In 1952 Philco Corporation was selected as prime contractor responsible for the production of test and (later) operational guidance and control sections. Philco's task was to achieve efficient production of the guidance and control section, redesigning its components where possible so as to obtain lower production costs without impairing performance.

In 1953 satisfactory performance of the Type A seeker head was achieved (wobble was eliminated), and, after a final demonstration of the Type B seeker, Type A was selected for the missile. A new gas-generating fuel was developed, and as a result the servo time lag was substantially reduced. The design of the chopper through which the infrared target radiation passes was greatly improved and the tracking of targets against some kinds of cloud background was demonstrated with the new choppers.

A total of 16 missiles, all launched from aircraft, were fired against targets (propeller-driven drones) in 1953. As a result of these tests successful development of a prototype Sidewinder -- one distinctly worth producing -- was considered achieved, except that fuze development was still incomplete. It was not until December 1953 that a requirement including performance specification and dimensions was issued by the Bureau of Ordnance. Although continuing improvement in the performance of most components was made a goal for the ensuing years, the basic design of the guidance and control section was "frozen" for production at Philco in March 1954.

Later in 1954, development of an influence fuze -- a completely self-contained, detachable section of the missile -- was completed at Eastman Kodak, and development of a contact fuze was completed by Bulova Research and Development Laboratories. Both fuzes went into production (and improved versions of both were later developed). Throughout 1954 there were further test firings, including night and high-altitude firings, using missiles made in the Philco model shop.

In 1955, design of the controlled-fragmentation warhead to be used in operational missiles was completed (previous missiles had been fired with dummy warheads) containing telemetering packages. A total of 88 more missiles were fired in 1955 including firings at supersonic jet targets. The last missiles fired came from the initial Philco pilot assembly line (rather than the model shop). The result of these tests was release of the missile, in January 1956, for Operational Development Force (OpDevFor) evaluation, the final step before introduction of the missile in fleet use.

The missile went through OpDevFor evaluation very quickly relative to other Navy missiles. This was due in part to the availability of field maintenance experts at NOTS who, during the latter phases of development, suggested minor design modifications affecting maintenance. Approval of design details affecting maintenance could be very quickly obtained.

Parallel Component Approaches by NOTS and by Contractors

There was extensive exploration of parallel approaches to certain components within NOTS and (in at least two important cases) by contractors. Different approaches to the lead-sulfide cell, the rotating chopper, the servo valves, the amplifier, the generator, and the propellant grain were, at some time in the course of development, pursued by groups at NOTS. The major set of parallel efforts was directed at the seeker head itself, as described above; two contractors and two groups at NOTS explored competing paths. Each of the contractors had a good expectation of obtaining a production contract should his approach prove to be the more successful. The same was

true of the two contractors developing influence and contact fuzes, on which some independent exploration was concurrently being performed at NOTS.

Development Cost, Development Time, and Military Value of Sidewinder

The annual development costs, up to 1957 (which marked completion of development of the improved Sidewinder 1A), are given in Table 9; the costs of development by contractors are shown as well as total costs. The cost of all test missiles up to but not including those delivered for OpDevFor evaluation are included.

This is a very low total development cost and a short development time relative to other air-to-air missiles. Sidewinder, moreover, proved a distinct success in its mission. It proved easily adaptable to a variety of aircraft. At no point in its development was it "matched" to any one of them. Improved versions of Sidewinder gradually broadened the mission in which it could be used.

Remarks

The development strategy pursued in the Sidewinder program seems to have been made particularly easy by the main organizational property of the project -- the great amount of authority given to the developing laboratory, and in particular to its head, who happened to be a gifted designer as well as the originator of Sidewinder. There was, in this case, an unusually intimate connection between strategy and organization.

Table 9
f
ANNUAL DEVELOPMENT COSTS, SIDEWINDER MISSILE,
1950-1957

Fiscal Year	Total Costs (thousands)	Contracts (thousands)
1950	100	
1951	365	
1952	1,360	
1953	2,930	626
1954	3,172	593
1955	4,291	818
1956	8,992	2,918
1957	<u>11,513</u>	<u>3,449</u>
Total	32,723	8,404

VII. CONCLUSIONS

We shall now examine the limitations and the possibilities of the preceding studies for the support of two hypotheses about development in the areas of technology we have considered. The first hypothesis is simply that major uncertainties are inevitable in development. Such uncertainties appear in all the cases considered. Predictions about the effort required to make a given design attain a specified performance are subject to large errors, especially at the start of development. Another study of a large group of projects, prepared by R. Summers, further supports this conjecture in a statistical manner.* In addition to such "technical uncertainties," there are often great "strategic uncertainties," for there typically occur outside the development project during the course of development, events that affect the relative desirability of the design being pursued: the relative value of different missions changes, fundamental state-of-the-art advances occur, and other projects turn out to have an unforeseen bearing on the project under study.

The second hypothesis is much harder to formulate and to test. It concerns the commitments made in the early phases of development, when crucial predictions are often necessarily inaccurate. Early commitments can be large or small. A large commitment permits one to start certain tasks that take a long time but that also require fairly detailed knowledge of the final form of the item. Such tasks are the preparation of production facilities and the selection and development of other items that are to be "mated" to the item in question in order to form a system. A small commitment may mean that these tasks have to be postponed; but a small commitment may also fairly quickly produce a large jump in the accuracy with which one can make predictions about the final form of the item under development.

* Robert Summers, Cost Estimates as Predictors of Actual Weapon Costs: A Study of Major Hardware Articles, The RAND Corporation, RM-3061-PR (Abridged) (forthcoming).

Thus two opposite extremes of development strategy can roughly be defined. It will be convenient to label the first extreme the "inflexible" strategy. This strategy takes very seriously the best predictions available at the start of development as to the effort required to achieve alternative, highly detailed specifications. One such set of specifications is chosen and large commitments to it are made, namely the commitments that would achieve rapid development if the predictions turned out to be correct. At the other extreme is a strategy we shall call "flexible." This avoids all but the broadest specifications to start with and makes no major commitment until a substantial jump in knowledge about the item being developed has been achieved. It concentrates at the start on achieving such a jump quickly by actually building and testing one or more crude versions of the item's least predictable parts. In this way it obtains predictions about the effort required to achieve alternative detailed specifications -- predictions generally much more accurate than the initial predictions on which the other strategy bases its large, early commitments.

When the developer who uses the inflexible strategy is lucky, all of the critical initial predictions turn out to be correct and there are no external events that cause him to revise his initial choices. He may then well have saved time and perhaps money as compared with his experience if he had used the second strategy. When the developer who uses the first strategy is unlucky the chosen specifications will turn out to be much harder to achieve than predicted; this, together with external events, will make the initial choice appear unfortunate. The developer may then be faced with the alternatives of abandoning the program or revising it; in either case parts of the initial heavy commitment (production facilities, programs aimed at another highly specified item to be mated to the item in question) are scrapped.

Suppose a developer undertakes a large number of programs in one of the areas of technology we have considered. His total experience, when all the programs are completed, is composed of the program's development times and costs, as well as the items finally obtained.

If he uses a fairly inflexible strategy in all the programs then, according to the hypothesis, his total experience will be inferior, in terms of his own preferences, to his experience if he had adopted a fairly flexible strategy. That is to say, the gains in time and money of the luckiest cases are outweighed by the revisions required in the unlucky cases.

There are a number of difficulties in using the preceding collection of histories to test this hypothesis.

(1) In the first place, few of the programs fall close to one or the other of the two extremes. Those that do not are not relevant to the hypothesis. It requires some subjective interpretation, moreover, to place even the less ambiguous programs in one category or the other.

In the case of radars, the side-looking radars and probably most of the World War II radars can plausibly be argued to fall close to the light commitment or flexible type of program. Some postwar radars, developed according to official procedures that required early commitments to highly specified designs, fall close to the heavy commitment or inflexible type of program. In the case of aircraft engines, the J-79, J-57, and J-75 lie close to the flexible type, while the J-40, the J-46, and the T-35 lie close to the inflexible type. Other engines are harder to place. In the case of fighter aircraft, the F-102 seems to be a program of the inflexible type. The F-100 is more ambiguous. On the one hand the initial commitment was a heavy one; on the other, the initial uncertainties were not as large as for most fighter aircraft programs because of the closely related aircraft preceding it. The F-104 program can reasonably be placed close to the flexible extreme.

In the case of the two bombers the situation is again not clear-cut. In the B-47 case the very early commitments were small and much information was obtained from the prototype phase. But immediately afterward, the program was accelerated and very heavy commitments were made to production. In the B-52 case there was a similar acceleration, even before the prototypes flew.

The Sidewinder, finally, falls as clearly as any program can into the flexible, low commitment category.

(2) A second difficulty in testing the proposed hypothesis is that of grouping the projects studied into significant pairs. In such a pair one project is of the inflexible type and the other of the flexible type, while both are similar with respect to the other circumstances that might explain major differences in the projects' total experiences. In the ideal "laboratory" test of the hypothesis the same project would be duplicated; it would be run once in a flexible manner and once in an inflexible manner, and the cost, time, and usefulness of the final item would be compared for the two versions. We can only hope to approximate this ideal test very crudely. One reasonable way of doing so is to find pairs in which one project was flexible and the other inflexible while both achieved the same order of technical advance. This approximation is only valid when the achievement of the same "order of technical advance" by the two projects also meant that the two achieved equally useful items. This would not be the case if external events drastically altered the usefulness of one of the two projects. In studying the project pairs "size of technical advance" is certainly a subjective matter, and one can probably do no better in determining it than to poll experts. Such a poll would probably reveal that the side-looking radar, the F-140, the Sidewinder, the J-57, the B-47, and the B-52 (all essentially flexible programs, as argued above) can each be paired with a program that was essentially inflexible and involved at least as great a technical advance. For the case of the J-57, the J-40 is probably an inflexible program involving a comparable advance; and for the case of the F-104, the F-102 is probably such a program. For the case of the side-looking radar at least one of the postwar radars probably plays a similar role. The inflexible member of each of these pairs took distinctly more time and money to develop than the flexible member. We have not here considered programs that pair up in the same way with the Sidewinder or with either of the bombers.

Some of the histories, then, fall into pairs that support the proposed hypothesis and others do not. None appears to fall into

pairs that directly conflict with the proposed hypothesis, that is, a pair of comparable programs, one clearly flexible, the other clearly inflexible, the latter having required distinctly less time and money.

(3) Finally, if the histories presented are to support the hypothesis they either should be roughly a random sample from each of the areas of technology or they ought to cover these areas completely (for the postwar years, say). Since histories often take a very great effort to compile, one has to settle for the former alternative. The preceding histories, however, were not random -- they were largely chosen because the projects seemed interesting and their study promised to be feasible.

Although our first hypothesis seems well supported, our second conjecture is far from settled, in any way or the other, by the preceding collection of histories. In at least one area (engines) the pairing of projects seems clear-cut enough to provide substantial support; in other areas many additional studies would be needed. We hope nevertheless to have sufficiently illustrated the project-history method so as to make clear its possibilities and its limitations.

We note, finally, that even if the project-history approach is far short of confirming a hypothesis, it may still be useful to the policy-maker. For the policy-maker, unlike the scientist, R&D, must make decisions here and now. If he has to choose now between two strategy types he must probably be content with incomplete groups of studies such as those presented here. While he cannot be certain his choice is the correct one such studies can make his uncertainty a great deal smaller.

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